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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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[FOURTH SERIES.]

JULY 1869.

I. *On some Electromagnetic Phenomena considered in connexion with the Dynamical Theory. By* The Hon. J. W. Strutt, *Fellow of Trinity College, Cambridge**.

It is now some time since general equations applicable to the conditions of most electrical problems have been given, and attempts, more or less complete, have been made to establish an analogy between electrical phenomena and those of ordinary mechanics. In particular, Maxwell has given a general dynamical theory of the electromagnetic field†, according to which he shows the mutual interdependence of the various branches of the science, and lays down equations sufficient for the theoretical solution of any electrical problem. He has also in scattered papers illustrated the solution of special problems by reference to those which correspond with them (at least in their mathematical conditions) in ordinary mechanics. There can be no doubt, I think, of the value of such illustrations, both as helping the mind to a more vivid conception of what takes place, and to a rough quantitative result which is often of more value in a physical point of view, than the most elaborate mathematical analysis. It is because the dynamical theory seems to be far less generally understood than its importance requires that I have thought that some more examples of electrical problems illustrated by a comparison with their mechanical analogues might not be superfluous.

As a simple case, let us consider an experiment first made by De la Rive, in which a battery (such as a single Daniell cell)

* Communicated by the Author.

† Philosophical Transactions for 1865.

whose electromotive force is insufficient to decompose water, becomes competent to do so by the intervention of a coil or electromagnet. Thus, let the primary wire of a Ruhmkorff coil be connected in the usual manner with the battery, and the electrodes of the voltameter (which may consist of a test-tube containing dilute sulphuric acid into which dip platinum wires) with the points where in the ordinary use of the instrument the contact is made and broken. There will thus be always a complete conducting circuit through the voltameter; but when the contact is made the voltameter will be *shunted*, and the poles of the battery joined by metal. Now when the shunt is open the battery is unable to send a steady current through the voltameter, because, as has been shown by Thomson, the mechanical value of the chemical action in the battery corresponding to the passage of any quantity of electricity is less than that required for the decomposition of the water in the voltameter. When, however, the shunt is closed, a current establishes itself gradually in the coil, where there is no permanent opposing electromotive force, and after the lapse of a fraction of a second reaches its full value as given by Ohm's law. If the contact be now broken, there is a momentary current through the voltameter, which causes bubbles of gas to appear on the electrodes, and which is often (but not, I think, well) called the extra current. Allowing the rheotome to act freely we get a steady evolution of gas.

To this electrical apparatus Montgolfier's hydraulic ram is closely analogous. The latter, it will be remembered, is a machine in which the power of a considerable quantity of water falling a small height is used to raise a portion of the water to a height twenty or thirty times as great. The body of water from the reservoir flows down a closed channel to the place of discharge, which can be suddenly closed with a valve. When this takes place, the moving mass by its momentum is able for a time to overcome a pressure many times greater than that to which it owes its own motion, and so to force a portion of itself to a considerable height through a suitably placed pipe. Just as the electromotive force of the battery is unable directly to overcome the opposing polarization in the voltameter, so of course the small pressure due to the fall cannot lift a valve pressed down by a greater. But when an independent passage is opened, the water (or electricity) begins to flow with a motion which continues to accelerate until the moving force is balanced by friction (resistance), and then remains steady. At the moment the discharge-valve is closed (or, in the electrical problem, the shunt-contact is broken), the water, by its inertia, tends to continue moving, and thus the pressure instantly rises to the value re-

quired to overcome the weight of the great column of water. The second valve is accordingly opened, and a portion of the water is forced up. Now the electrical current, in virtue of self-induction, can no more be suddenly stopped than the current of water; and so in the above experiment the polarization of the voltameter is instantly overcome, and a quantity of electricity passes.

If no second means of escape were provided for the water in the hydraulic ram, the pipe would in all probability be unable to withstand the shock, and in any case could only do so by yielding within the limits of its elasticity, so as gradually, though of course very quickly, to stop the flow of water. The bursting of the pipe may properly be compared to the passage of a spark at the place where a conductor carrying an electric current is opened. Just as the natural elasticity of the pipe or the compressibility of the air in a purposely connected air-vessel greatly diminishes the strain, so the electrical spark may be stopped by connecting the breaking-points with the plates of a condenser, as was done by Fizeau in the induction-coil. Contrary to what might at first sight have been expected, the fall of the primary current is thus rendered more sudden, and the power of the instrument for many purposes increased. Of course the spark is equally prevented when the breaking-points are connected by a short conducting circuit, as in our experiment by the voltameter. In fact the energy of the actual motion which exists the moment before contact is broken is in the one case transformed into that of the sound and heat of the spark, and in the other has its equivalent partly in the potential energy of the decomposed water, partly in the heat generated by the passage of the momentary current in the voltameter branch.

The experiment will be varied in an instructive manner if we replace the voltameter by a coil (with or without soft iron), according to the resistance and self-induction of the latter. In order to know the result, we must examine closely what takes place at the moment when contact is broken. The original current, on account of its self-induction or inertia, tends to continue. At the same time the inertia in the branch circuit tends to prevent the sudden rise of a current there. A force is thus produced at the breaking-points exactly analogous to the pressure between two bodies, which we will suppose inelastic, one of which impinges on the other at rest. The pressure or electrical tension continues to vary until the velocities or currents become equal. All this time the motion of each body or current is opposed by a force of the nature of friction proportional to the velocity or current. Whether this resistance will affect the common value of the currents (or velocities) at the moment

they become equal, will depend on its magnitude as compared with the other data of the problem.

There is for every conducting circuit a certain time-constant which determines the rapidity of the rise or fall of currents, and which is proportional to the self-induction and conductivity of the circuit. Thus, to use Maxwell's notation, if L and R be respectively the coefficient of self-induction and the resistance, the time-constant is $\frac{L}{R}=\tau$. If the current c exist at any moment in the circuit and fall undisturbed by external electromotive force, the value at any time t afterwards is given by $x=c.e^{-\frac{t}{\tau}}$. Any action which takes place in a time much smaller than τ will be sensibly unaffected by resistance.

We see, then, that we may neglect the effects of resistance during the time of equalization of the currents, provided that the operation is completed in a time much smaller than the time-constants of either circuit. And this I shall suppose to be the case. The value of the common current or velocity at the moment the impact is over will of course be given by the condition that the momentum, electromagnetic or ordinary, is unchanged. If L and N be the coefficients of self-induction for the main and branch circuits respectively, x and X the original and required currents, the analytical expression of the above condition is

$$(L+N)X=Lx,$$

or

$$X=\frac{L}{L+N}x.$$

It is here supposed that there is no sensible mutual induction between the two circuits.

The spark is the result of the excess of the one current over the other, and lasts until its cause is removed. Its mechanical value is the difference between that of the original current in the main circuit and that of the initial current in the combined circuit, and is expressed by

$$\tfrac{1}{2}Lx^2-\tfrac{1}{2}(L+N)X^2;$$

or if the value of X be substituted,

$$\tfrac{1}{2}Lx^2-\frac{L}{L+N}.$$

Exactly the same expression holds good for the heat produced during the collision of the inelastic bodies, which is necessarily equal to the loss of ordinary actual energy, at least if the per-

manent change of their molecular state may be neglected. From the value X the current gradually increases or diminishes to that determined according to Ohm's law, by the resistance of the combined circuit. It may be seen from the expression just found that the resistance of the branch may be varied without affecting the spark, provided always that it is not so great in relation to the self-induction as to make the time-constant comparable in magnitude with the duration of the spark. The spark depends only on the comparative self-induction of the branch circuit, being small when this is small, and when this is great approximating to its full value $\frac{1}{2}Lx^2$.

These results are easily illustrated experimentally. I have two coils of thick wire belonging to an electromagnet, which for convenience I will call A and B. Each consists of two wires of equal length, which are coiled together. These may be called A_1 A_2, B_1 B_2. When A_1 A_2 are joined consecutively, so that the direction of the current is the same in the two wires, we have a circuit whose self-induction is four times that of either wire taken singly. But if, on the contrary, the current flows opposite ways in the two wires, the self-induction of the circuit becomes quite insensible.

The main circuit may be composed of the wire A_1 (A_2 remaining open) into which the current from a single Daniell cell is led, and which can be opened or closed at a mercury cup. One end of the branch circuit dips into the mercury while the other communicates with the wire whose entrance or withdrawal from the cup closes or opens the main circuit. In this way the coils of the branch may be said to be *thrown in* at the break.

If the branch is open, we obtain at break the full spark, whose value is $\frac{1}{2}Lx^2$. If the wire B_1 be thrown in, the spark is still considerable, having approximately the value $\frac{1}{4}Lx^2$ for $N=L$. And if B_1 B_2 are thrown in, so that the currents are parallel, the spark is still greater and is measured by $\frac{1}{2}Lx^2 \times \frac{4}{5}$. But if the currents are opposed, the spark disappears, because now $N=0$; so that the addition of the wire B_2, whereby the resistance of the branch is doubled, diminishes the spark. It is true that to this last case our calculation is not properly applicable, inasmuch as the time-constant of the branch is so exceedingly small. But it is not difficult to see that in such a case (where the self-induction of the branch may be neglected) the tension at the breaking-points, or more accurately the difference of potential between them, cannot exceed that of the battery more than in the proportion of the resistances of the branch and main circuits, so that it could not here give rise to any sensible spark. Soft iron wires may be introduced into the coils in order to exalt the effects; but solid iron cores would

allow induced currents to circulate which might interfere with the result.

In this form of the experiment there was no sensible mutual induction between the coils A and B. Should there be such, the result may be considerably modified. For instance, let the wire A_2 be thrown at the break into the circuit of A_1 and the battery. This may happen in two ways. If the connexions are so made that the currents are parallel in A_1 A_2, there will be no sensible spark; but if the directions of the currents are opposed, the spark appears equal to the full spark $\frac{1}{2}Lx^2$.

And this is in accordance with theory. The current X is given by the same condition as before, which leads to the equation

$$Lx + Mx = (L + 2M + N)X,$$

M being the coefficient of mutual induction between the two circuits. The spark is therefore

$$\tfrac{1}{2}Lx^2 - \tfrac{1}{2}(L + 2M + N)X^2 = \frac{x^2}{2}\frac{L - M}{2}, \text{ as } N = L.$$

Now in the first-mentioned connexion $M = L$ very nearly, and in the second $M = -L$; so that the observed sparks are just what theory requires.

With regard to those electrical phenomena which depend on the mutual induction of two circuits, it may be remarked that it is not easy to find exact analogues in ordinary mechanics which are sufficiently familiar to be of much use as aids to conception. A rough idea of the reaction of neighbouring currents may be had from the consideration of the motion of a heavy bar to whose ends forces may be applied. If when the bar is at rest one end is suddenly pushed forwards in a transverse direction, the inertia of the material gives the centre of gravity in some degree the properties of a fulcrum, and so the other end begins to move backwards. This corresponds to the inverse wave induced by the rise of a current in a neighbouring wire. If the motion be supposed infinitely small, so that the body never turns through a sensible angle, the kinetic energy is proportional to

$$\tfrac{1}{2}(a^2 + k^2)x^2 + \tfrac{1}{2}(b^2 + k^2)y^2 + (ab - k^2)xy,$$

where a and b are the distances of the driving-points (whose velocities are x and y) from the centre of gravity, k^2 the radius of gyration about the latter point. This corresponds to the expression for the energy of the electromagnetic field due to two currents,

$$\tfrac{1}{2}Lx^2 + Mxy + \tfrac{1}{2}Ny^2;$$

and if we imagine the motion of the driving-points to be resisted by a frictional force proportional to the velocity, we get a very tolerable representation of the electrical conditions.

Or we may take an illustration, which is in many respects to be preferred, from the disturbance of a perfect fluid, by the motion of solid bodies in its interior. Thus if in an infinite fluid two spheres move parallel to each other and perpendicularly to the line joining them, and with such small velocities that their relative position does not sensibly change, the kinetic energy may as usual be expressed by

$$\tfrac{1}{2}\mathrm{L}x^2+\mathrm{M}xy+\tfrac{1}{2}\mathrm{N}y^2,$$

x,y denoting the velocities of the two spheres, and L, M, N being approximately constants*. When the spheres move in the same direction, the reaction of the fluid tends to press them together; but if the motions are opposed, the force changes to a repulsion. We see here the analogues of the phenomena of attraction and repulsion discovered by Ampère. If when all is at rest a given velocity is impulsively impressed on one sphere, the other immediately starts backwards, and, as Thomson † has shown, with uch velocity that the energy of the whole motion is the least possible under the given condition.

This theorem is general, and leads directly to the solution of a large class of electrical problems connected with induction; for whenever a current is suddenly generated in one of the circuits of a system, the initial currents in all the others are to be determined so as to make the energy of the field a minimum. These initial currents are formed unmodified by resistance whenever the electromotive impulses to which they owe their existence last only for a time which may be regarded as vanishingly small compared with the time-constants of the circuits. The sudden fall of a current when a circuit is opened generates the same currents, except as to sign, in neighbouring circuits as those due to a rise of the first current, and the condition as to sufficient suddenness is more generally fulfilled; at the same time it is more convenient in explaining the theory to take the case of the establishment of the primary current.

Suppose, then, that in the wire A_1 of our coil a current x is suddenly generated, while the ends of A_2 are joined by a short wire. The condition of minimum energy is obviously fulfilled if there arise in A_2 a current represented by $-x$; for then the energy of the field is approximately zero. But if the self-induction of the wire joining the ends of A_2 be sensible, the annihilation of the energy can no longer be perfect. Thus, let the circuit of A_2 be completed by B_1 B_2, then the general expression for the energy of two currents becomes in this case

$$\tfrac{1}{2}\mathrm{L}x^2+\mathrm{L}xy+\tfrac{1}{2}\mathrm{L}y^2\times(5 \text{ or } 1,$$

* Thomson and Tait's 'Natural Philosophy,' pp. 262, 264.

† Thomson and Tait, p. 225.

according to the connexions); and the value of y for which this is a minimum is $-x\,(1 \text{ or } \frac{1}{5})$. In the first case, the exterior part of the induced circuit having no sensible self-induction, takes away nothing from the initial current; but in the second there is a reduction to one-fifth. On the other hand, it makes no difference to the total current $\left(-\frac{M}{S}x\right)$*, as measured by the deflection of the galvanometer-needle, which way the connexion is made; for the smaller initial current, in virtue of its greater inertia, sustains itself proportionally longer against the damping action of resistance, which is the same in the two cases. The heating-power and the effect on the electrodynamometer, which depend on the integral of the square of the current while it lasts $\left(\frac{1}{2}\frac{M^2}{NS}x^2\right)$, will be different; but the easiest proof of the diversity of the currents is to be had by comparing their powers of magnetizing steel.

Thus, if we include in the induced circuit a magnetizing spiral in which is placed a new sewing-needle, we shall find an immense difference in the magnetization produced by a break-induced current, according as its direction is the same or otherwise in the wires B_1 B_2. In the actual experiment the diluted current was unable, even after several repetitions, to give the needle any considerable magnetization (the vibrations were only about three per minute), while after one condensed current the needle gave sixteen, raised by repetition to nineteen †. A new needle submitted to the action of several condensed currents also gave nineteen per minute. The magnetic moments, which are as the squares of these numbers, show a still greater disproportion.

The truth seems to be that the time required for the permanent magnetization of steel is so small as compared even with the duration of our induced currents, that the amount of acquired magnetism depends essentially on the initial or maximum current without regard to the time for which it lasts.

The increased heating-effect when the two parts of the current in B are opposed in direction is, of course, at the expense of the spark in the mercury-cup. The mechanical value of the spark is the difference between the values of the currents which exist at the moments before and after the breaking of the contact, and

$$=\tfrac{1}{2}Lx^2-\tfrac{1}{2}Ny^2 \quad =\tfrac{1}{2}x^2\left(L-\frac{M^2}{N}\right) \quad =\tfrac{1}{2}x^2\left(L-\frac{L^2}{N}\right) \text{ nearly.}$$

* R, S are the resistances of the primary and secondary circuits respectively.

† These were *complete* vibrations.

Now, according to the connexions, $N = L$ or $5L$; and so in the first case the spark disappears, while in the second it falls short of the full spark by only one-fifth.

While considering the dynamics of the field of two currents, I noticed that the initial induced current due to a sudden fall of a given current in the primary wire is theoretically greater the smaller the number of terms of which the secondary consists; for in calculating the energy of the field, it makes no difference whether we have a current of any magnitude in a doubled circuit, or twice that current in a single circuit. The same conclusion may be arrived at by the consideration of the analytical expression for the initial induced current

$$y_0 = -\frac{M}{N}x;$$

for if the secondary circuit consists essentially of a single coil of n terms, we have, *cæteris paribus*, $M \propto n$, while $N \propto n^2$, so that $y_0 \propto \frac{1}{n}$. The whole induced current $\int y dt \propto M \propto n$. Intermediate to these is the heating-effect $\int y^2 dt$, which $\propto \frac{M^2}{N}$, and is therefore independent of n. Thus it was evident that neither the galvanometer nor electrodynamometer were available for the verification of this rather paradoxical deduction from theory, at least without commutators capable of separating one part of the induced current from the rest. On the other hand, it appeared probable that the smaller total current, in virtue of its greater maximum, might be the most powerful in its magnetizing action on steel.

With the view of putting this idea to the test of experiment, I bound three wires of ·001 inch diameter, and about 20 feet long, together into a coil whose opening was sufficient to allow it to pass over the coil A. The ends of the wires were free, so that they could be joined up in any order into one circuit, which was also to contain the magnetizing spiral. It is evident that if the currents are parallel in the three wires (an arrangement which I will call *a*), then

$$M = 3M_0, \quad N = 9N_0,$$

M_0 N_0 being the values of the induction-coefficients for *one* wire; while if in the two wires the current flows one way round and in the third the opposite (*b*), we shall have $M = M_0$, $N = N_0$. Inasmuch as the self-induction of the magnetizing spiral was relatively very small, these may be regarded as the induction-coefficients for the secondary circuit as a whole. This arrangement was adopted in order that there might be no change in the resistance

in passing from one case to the other. The primary current was excited by a Daniell cell in the two wires of A arranged collaterally, and was interrupted at a mercury-cup. The needle was submitted to the *break* induction-currents only—although the make currents had no perceptible magnetizing-power, on account of the relatively large time-constant of the primary circuit, and the consequent slow rise of its current to the maximum.

On actually submitting a new needle to the current *a*, I obtained after one discharge 12 vibrations (complete) per minute, a number raised after several discharges to 15. On the other hand, a new needle after one discharge *b* gave only 5 per minute, and was not much affected by repetition. The last needle being now submitted to discharge *a* gave $8\frac{1}{2}$, and after several 12. Other trials having confirmed these results, there seemed to be no doubt that the current *a* was the most efficient magnetizer. There remained, however, some uncertainty as to whether the time-constant, especially in *b*, was sufficiently large relatively to the time for which the spark at the mercury cup lasted to allow of the initial current being formed undiminished by resistance. In order to make the fall of the primary current more sudden, I connected the breaking-points with the plates of a condenser belonging to a Ruhmkorff coil, and now found but little difference between the magnetizing-powers of *a* and *b*. Seeing that the theoretical condition had not been properly fulfilled, I prepared another triple coil of much thicker wire, and, for greater convenience, arranged a mercury-cup commutator, by means of which it was possible to pass at once from the one mode of connexion to the other. The magnetizing spiral was still of fine wire coiled, without any tube, closely over the needle, and its ends were soldered to the thicker wire of the triple coil.

The experiment was now completely successful. Out of the large number of results obtained, the following are selected as an example. A new needle was submitted to the break discharge of arrangement *b*, and gave,

After	1	discharge,	19	per minute.
„	3	„	23	„
„	6	„	24	„

Another needle was now taken and magnetized by discharge *a*. It gave,

After	1	discharge,	11	per minute.
„	3	„	12	„
„	10	„	$12\frac{1}{2}$	„

On submitting this needle, which had received all the mag-

netism that *a* could give it, to current *b*, I obtained,

After 1 discharge, 21 per minute.
" 3 " 24 "

In fact it was the general result of the experiments that more magnetism is always given to the needle by arrangement *b* than by *a*. In order, however, that the difference may be striking, it is advisable not to approach too nearly the point of magnetic saturation. The numbers quoted were obtained with the condenser, which was still necessary, in order to make the break sufficiently sudden. I have no doubt, however, that it might have been dispensed with had the triple coil consisted of a larger number of turns.

The circumstances of this experiment are in some degree represented by supposing, in the hydrodynamical analogue, one of the balls to vary in size. When a given motion is suddenly impressed on the other ball, the corresponding velocity generated in the first would vary inversely with its magnitude; for the larger the ball the greater hold, as it were, would it have on the fluid.

It is interesting also to examine the influence of neighbouring soft iron on the character of the induced current. This influence is of two sorts; but I refer here to the modifications produced by the magnetic character of iron. The circulation of induced currents in its mass may generally be prevented from exercising any injurious influence on the result by using only wires, or fragments of small size. The proximity of soft iron always increases the coefficient of self-induction N, while M may be either increased or diminished. The latter statement is true also for the initial current y_0, which is proportional to $\frac{M}{N}$. For the two wires of the coil A, however, it is easy to see that M and N are approximately equal, whether there be soft iron in their neighbourhood or not. Thus, if A_1 be connected with a Daniell cell while the circuit of A_2 is completed by the magnetizing spiral, the magnetism acquired by the needle, after a break-induced current, is not much altered, even if a considerable number of iron wires are placed in the coil. The total current is increased fifteen times or more; but this is because the current lasts longer, the maximum or initial value being no greater than before. This experiment strikingly illustrates the comparative independence of the magnetizing effect of a current on its duration. It seems probable *à priori*, and is partly confirmed by some of my experiments, that this is more especially true if we take the limiting magnetism which

an induced current can produce, after repetition, as the measure of its magnetizing powers.

The same kind of reasoning may be applied to more complicated problems. As an example, we may recur to a former combination, in which the primary current is excited in the wire A_1, while the secondary circuit includes A_2, B_1, and the magnetizing spiral. The initial current y_0, on which, as we have seen, the magnetizing power mainly depends, will be greatly increased if the ends of the wire B_2 are joined so as to make a tertiary circuit; for a current in B_2 is developed, which, being equal and contrary to that in B_1, neutralizes its action on the magnetic field, and so allows the energy, immediately after the sudden rise of the current x in A_1, to be vanishingly small, exactly as when the secondary circuit consisted of A_2 alone. The effect of closing B_2 is therefore to increase the current y_0 from $-\frac{1}{2}x$ to $-x$, and at the same time to produce a new current denoted by $+x$ in B_2 itself. The following were some of the experimental results:—

B_2 open	A new needle,			
	After 1	break-discharge, gave	$7\frac{1}{2}$	per minute.
	„ 8	„	„ 9	„

On closing B_2 we had, with the same needle,

After 1	discharge,	15	per minute.
„ 8	„	17	„

A *new* needle gave,

After 1	discharge,	17	per minute.
„ 8	„	19	„

Another new needle in the tertiary circuit gave,

After 1	discharge,	16	per minute.
„ 4	„	19	„
„ 8	„	$19\frac{1}{2}$	„

The magnetizing spiral was here removed from the secondary to the tertiary circuit; and although its resistance was by no means relatively small, the results are none the less comparable; for in this experiment resistances (within limits) are of no account, and the *self-induction* of the spiral was quite insensible.

Had there been twenty coils A B C D . . . . . . similar to A B, with the wires B_2 C_1, C_2 D_1, &c. connected, as in the experiment just described, the magnetizing power of the current in the last would not, I imagine, be much less than in the first; for the condition of minimum energy would still be fulfilled by currents in the series of coils all equal in numerical value, and alternately opposite in algebraic sign. On this subject much

confusion seems to have prevailed, as shown by the numerous inquiries into the *direction* of the induced currents of high orders. The currents, as a whole, at least after the first, cannot properly be said to have any direction at all, as they involve, when complete, no transfer of electricity in any direction. Nevertheless the positive and negative parts are not similar; and if they were, one must necessarily precede the other; so that in this way directional effects may be produced. The magnetizing power, for instance, depends essentially on the initial maximum magnitude of the induced current, and is probably but little affected by the character of the diluted but comparatively long-continued remaining parts. This being understood, the alternately opposite magnetizations observed by Henry in a series of induced currents of high order, is an immediate consequence of the dynamical theory.

The circuits being denoted by the numbers 1, 2, 3, . . . , let the coefficient of mutual induction between 2 and 3 be denoted by (2 3), and of self-induction of 2 by (2 2), and so on. The result is only generally true when there is no mutual induction except between immediate neighbours in the series; and it will therefore be supposed that

$$(1\,3),\quad (1\,4),\quad (1\,5)\ldots(2\,4)\ldots$$

vanish, as indeed they practically would in the ordinary arrangement of the experiment. The energy of the field is given by

$$\begin{aligned} \mathrm{E} &= \tfrac{1}{2}(1\,1)x_1^2+\tfrac{1}{2}(2\,2)x_2^2+\tfrac{1}{2}(3\,3)x_3^2+\ldots \\ &\quad +(1\,2)x_1x_2+(2\,3)x_2x_3+(3\,4)x_3\,x_4+\ \ldots \end{aligned}$$

Here x_1 is the given current in the first circuit, and x_2, x_3, . . . are to be determined so as to make E a minimum. Now, E being homogeneous in x_1, x_2, . . . , we have identically

$$2\mathrm{E}=x_1\frac{d\mathrm{E}}{dx_1}+x_2\frac{d\mathrm{E}}{dx_2}+\ldots$$

And since, when E is a minimum,

$$\frac{d\mathrm{E}}{dx_2},\ \frac{d\mathrm{E}}{dx_3},\ \ldots \text{ all vanish,}$$

we see that

$$2\mathrm{E}\ (\text{min.}) = x_1\frac{d\mathrm{E}}{dx_1}=(1\,1)x_1^2+(1\,2)\,x_1x_2.$$

But if x_2, x_3, . . . had been all zero, 2E would have been equal to $(1\,1)\,x_1^2$. It is clear therefore that $(1\,2)x_1\,x_2$ is negative; or, as (1 2) is taken positive, the sign of x_2 is the opposite of that of x_1.

Again, supposing x_1 x_2 both given, we must have, when E is a minimum,

$$\frac{d\mathrm{E}}{dx_3},\ \frac{d\mathrm{E}}{dx_4},\ldots=0,$$

and thus

$$\begin{aligned}2\mathrm{E}\,(\text{min.}) &= x_1[(1\ 1)x_1+(1\ 2)x_2]\\ &\quad +x_2[(1\ 2)x_1+(2\ 2)x_2+(2\ 3)x_3]\\ &=[1\ 1)x_1^2+2(1\ 2)x_1\,x_2+(2\ 2)x_2^2+(2\ 3)x_2x_3.\end{aligned}$$

As before, 2E might have been

$$(1\ 1)x_1^2+2(1\ 2)x_1\,x_2\ +(2\ 2)x_2^2;$$

and therefore the minimum value is necessarily less than this, and accordingly the signs of x_2 and x_3 are opposite. This process may be continued, and shows that, however long the series, the initial induced currents are alternately opposite in sign. In any definite example, the actual values of the initial currents are to be found from the solution of the linear equations

$$\frac{d\mathrm{E}}{dx_2}=0,\quad \frac{d\mathrm{E}}{dx_3}=0,\ldots;$$

but the *sign* of the result does not appear at once from the form of the expression so obtained. In order to exhibit it, it is necessary to introduce a number of relations which exist between the induction-coefficients, and which are the analytical expression of the fact that the energy is always positive, whatever may be the values of x_2, x_3, . . .

It has been assumed throughout that the time of rise or fall of the current in the primary wire is very small as compared with the time-constants of the other circuits. In the case of coils, such as are generally used in induction-experiments, and which are not clogged by great external resistances, this condition is abundantly fulfilled at the break of the voltaic current*. The time of rise depends more on the nature of the circuit, but may be made as small as we please by sufficiently increasing the resistance in proportion to the self-induction; of course, in order to get an equally strong current, a higher electromotive force must be employed. In this way the rise may be made sufficiently sudden to fulfil the condition. Indeed, with a battery intense enough the rise of the current at *make* may become more sudden

* A rough measurement by Maxwell's method (Phil. Trans. 1865) gave for the time-constant of the circuit composed of the two wires of coil A ·0023″. The time-constant is the same whether the wires are collateral or consecutive, the greater self-induction of the latter arrangement being balanced by its greater resistance. For *one* wire only, the time-constant would be *half* the above.

than the fall when contact is broken. In some of Henry's experiments this seems actually to have occurred. Thus, with a single cell as electromotor, he found the shock at make barely perceptible; but when the battery was increased to thirty cells, the shock became more powerful at make than at break.

And here I must bring this rather disjointed paper to a close.

Terling Place, Witham,
June 1.

II. *On the Function of the Blood in Muscular Work. By* W. H. Broadbent, *M.D., Lecturer on Physiology at St. Mary's Hospital Medical School**.

IN the Philosophical Magazine for May 1867 there is a paper under the title given above by Mr. C. W. Heaton, Professor of Chemistry at Charing-Cross Hospital, the purport of which is to show that the oxidation which yields the force exerted by the muscles is intravascular, or that muscular force is generated entirely from the blood and within the blood-vessels. As this communication is considered by some eminent physiologists to have established the hypothesis that the blood itself is both the source and the seat of all the chemical change by which force is developed in the animal organism, it is desirable to examine whether the considerations on which it is based are really so conclusive.

The point in question is whether the oxidation which evolves muscular force is intravascular or extravascular. The arguments employed by Professor Heaton are as follows:—

1. "If the oxidation of muscle is effected in the tissue itself, it is clearly necessary to suppose either that the oxygen, upon the stimulus of the motor nerves, leaves its combination in the corpuscle, traverses the walls of the capillary in company with the outgoing stream of nutrient fluid, and only enters into new combinations when it has passed to some comparatively distant muscle-fibre, or else that the corpuscle itself liquefies and passes out bodily through the thin membrane with its loosely combined oxygen. . . . . Any oxygen which passes out into the tissues must obviously pass in solution in the exudate."

2. The lymph collected from the tissues and again poured into the blood may be taken as the measure of the exudate which passes out of the capillaries into the structures; and it is shown by careful calculation, exaggerating both the amount of exudate and the proportion of oxygen dissolvable in it, that the quantity of oxygen which could thus be carried to the tissue is utterly

* Communicated by the Author.

inadequate to effect the oxidation required for the evolution of the force actually exerted by the muscles.

The entire question thus turns on the assumption that oxygen can leave the capillaries only by passing through the thin membrane of which they consist, in solution in a fluid exudate. The necessity for a current of fluid to convey the oxygen is supposed to arise from the fact that the oxygen, being in solution in the blood, carries with it its solvent in passing through the capillary wall—just as in dialysis the saline matter is accompanied by the water in which it is dissolved. But this view of the process leaves entirely out of consideration the fact that if oxygen leaves the capillaries, the products of oxidation (carbonic anhydride &c.) must enter them; and when two diffusible substances are in solution on opposite sides of a thin membrane, the adverse currents of the common solvent more or less neutralize each other, and there is interchange of the dissolved matters with comparatively little movement of fluid.

If oxygen can leave the blood only in solution in a current of fluid, how, it may be asked, does it enter the blood in the lungs? It would seem that there ought to be a stream of fluid setting in from the air-cells into the pulmonary capillaries; and this would be required were it not that, as the oxygen enters the blood, carbonic anhydride leaves it. On the hypothesis that oxidation is extravascular, the exchange of oxygen for carbonic anhydride is effected very similarly in the pulmonic and systemic capillaries. In the lungs the oxygen is dissolved in the moisture of the walls of the air-sacs; there is thus outside the capillary membrane fluid containing oxygen, while in its interior is moving the blood charged with CO^2; interchange of the two gases consequently takes place. In the systemic capillaries the blood is oxygenated, while outside the capillaries is the interstitial fluid of the textures containing the CO^2 which has resulted from oxidation. The conditions under which interchange will occur are here again realized; the capillary wall stands between two fluids, one charged with O, the other with CO^2. Here, however, the O is in the blood, instead of CO^2 as in the lungs. It is not the affinity of a distant fibre for oxygen which overcomes the weak "molecular combination" of this gas with the blood-corpuscles, but the presence of CO^2 in the surrounding fluid; and the affinity of O and CO^2 for hæmatoglobin is so nearly balanced, that they mutually displace each other according as one or the other predominates.

It is thus evident that, supposing the oxidation to take place outside the capillaries, the oxygen does not require a stream of fluid to convey it to the tissues; and this being the case, the calculation by which it is shown that the exudate is insufficient for the purpose has no bearing whatever on the question whether

the oxidation is intra- or extravascular. This consequently has to be decided on other grounds; and the evidence in favour of the view that the oxidation takes place outside the capillaries preponderates greatly. In muscle, besides the proper muscular fibre with its connective tissue and the capillaries, there is an interstitial fluid (the "muscular juice"), which Claude Bernard calls the "*milieu*" of the fibre, and which may be regarded as a medium common to the fibre and the vessel. On the one hand, it is by the reaction between the fibre and this fluid which surrounds and saturates it that the chemical change takes place (oxidation or its equivalent) by which the force is evolved; on the other hand, this fluid being separated from the blood only by the thin capillary wall, the most perfect equalization of their diffusible constituents must take place by osmosis, oxygen passing from the blood into the interstitial fluid, and products of oxidation from this fluid into the blood; so far, then, as the supply of oxygen is concerned, the muscular juice is equivalent to the blood. Were intravascular oxidation the source of muscular force, the evolution of the force must cease absolutely on the supply of blood being cut off. We find, on the contrary, that a muscle continues to contract for some time after its removal from the body, showing that force (or, in other words, oxygen and oxidizable material) is stored up in the muscle; and it is further found that after frequent and sustained contraction the muscular juice is changed in composition. We find, again, that muscular contractility survives removal longest in cold-blooded animals, whose blood contains a minimum of oxygen; and when a warm-blooded animal is brought into a state analogous to that of reptiles, its blood being rendered venous and its temperature greatly lowered, its muscles also retain their contractility, as has been shown by Claude Bernard's "lapin à sang froid," in which the above conditions are induced by section of the cervical spinal cord. It is perhaps scarcely necessary to notice a difficulty in the hypothesis of Professor Heaton; but it might fairly be asked how force evolved within the capillary is transmitted to the "comparatively distant fibre" by which it is manifested.

Oxidation has been spoken of in this discussion as the source of muscular work without any qualification; but it should be understood that there is an essential difference between the mode of oxidation which yields the animal heat, and that which affords mechanical work or nerve-force. While heat is evolved continuously and uniformly, nervo-muscular action takes place intermittently, abruptly, and with varying intensity on the application of a "stimulus," *i. e.* the oxygen and oxidizable matter being in presence, the combination only occurs when some impulse is given. It is thus not a simple case of combination of oxygen

with a combustible, but the rearrangement of the elementary constituents of a complex molecule in a state which, for want of a better term, I have called elsewhere* "chemical tension." In the communication alluded to the evolution of nerve-force only was considered, and the conclusion here stated was reached deductively, but experimental confirmation is afforded by Hermann's researches on the chemical changes attending muscular action.

III. *On Vital Force according to Age, and the "English Life Table."* *By* Thomas Rowe Edmonds, *B.A. Cantab.*†

OBSERVATIONS on the vital force of man at different ages from birth are all of modern date. The idea of the existence in every population of a law of vital force according to age was not entertained by mankind until near the end of the seventeenth century. The embodiment of this idea in a "Table of Mortality" was first made about the year 1693, by our countryman, Dr. Halley. The form of the Table of mortality adopted about the year 1738, and continued in use to the present time, may be described as follows :—Such Table consists of three columns. The heading of the first column is "Age," of the second column "Living," and of the third column "Dying." The numbers in the *first* column denote completed years of age from birth-time, beginning at age 0 and ending, say, at 99 years. The numbers in the *second* column denote the living or survivors at any completed year of age out of a given number born or living at the age 0. Lastly, the numbers in the *third* column denote the numbers dying during the year of age next following the completed year marked, in the same horizontal line, in the first column.

In a Table of mortality, if the numbers in the column of "*Living*" be represented by the letter P, the numbers in the column of "*Dying*" will be represented by ΔP, for a unit of time or age taken to be one year. If the time or age be reckoned from birth, we shall have at any age t the quantity $\frac{\Delta P_t}{P_t}$ to represent the ratio of the numbers *dying* during the $(t+1)$th year of age to the numbers *living* at the beginning of the same year of age. If the intervals of age, instead of being each one year, be diminished indefinitely, we shall have to substitute the differential of P_t or $d.P_t$ for ΔP_t in the above ratio. We shall then obtain $\frac{d.P_t}{P_t}$, or $d.\log_e P_t$ for the expression of the ratio of the

* Proceedings of the Royal Society, June 1868.
† Communcated by the Author.

dying to the living, during an infinitely small given time dt, at the precise age t years, t being either a whole number or fractional. If a simple function of the variable t can be discovered which will represent $d \cdot \log_e P_t$ at all ages, then by integration the value of $\log_e P_t$, and consequently of P_t, may be determined for all ages. It may be useful here to state that the ratio of the dying to the living for an indefinitely small given time dt, at the exact age t, represents the *force of mortality* at that age—also that the *vital force* at any age t is represented by the reciprocal of the force of mortality at the same precise point of age.

A Table of mortality for a particular population is a mode of exhibiting the ratio of the dying to the living in that population for every year of age from birth-time to the end of life. The knowledge of this series of annual ratios (which is the foundation of every true Table of mortality) can be obtained only by observations of the contemporary numbers living and dying at every interval of age. In the making of such observations, the intervals of age ought to be quinquennial at all ages above five years, biennial at ages above one and less than five years, quarterly in the first year of age, and monthly in the first quarter of year from birth. No observation of the kind now described was known to the public until near the end of the eighteenth century, when the Sweden Table of mortality constructed by Dr. Richard Price was published. Dr. Halley's Table for Breslau, as well as all other Tables of mortality for specific populations, which had been constructed previously, were defective and not to be relied upon through not being founded on the requisite data mentioned above. These defective Tables had been deduced from observations made only on the registered number of *deaths* at different ages belonging to the several populations, without any observation or enumeration of the contemporary numbers *living* at the same ages. The defects inseparable from such Tables were partially remedied in various ways. Populations were selected for observation in which the numbers living at all ages were nearly stationary, and in which the annual births had been nearly equal to the annual deaths for a long period of time. Then the supposition was made that the living population at each interval of age was constant and not increased or diminished by migration. Lastly, corrections were introduced to rectify manifest deviations from the assumed condition of a stationary population at every interval of age.

Observations made correctly, and in the proper form for determining the vital force of man at different ages, are very few in number. In the first rank are the observations of the living and dying, according to age, of the population of Sweden, commencing about the year 1750 and continued to the present

time. In these observations the ages and numbers of the contemporary living and dying are given for quinquennial intervals at all ages above five years of age, and for biennial and annual intervals below that age. Next in time and very high in rank comes the observation of the living and dying, according to age, of the population of Carlisle, made for the nine years ending with the year 1787. This observation was made spontaneously by a private individual, Dr. Heysham, without aid in money or labour from the public. This observation, on the vital force, according to age, of the population of a town of no great magnitude, is in accuracy and form of so high a character, that it is equal in value to any ordinary observation of the same kind made on a population a hundred times as great in extent. Last in time comes the observation on the living and dying, according to age, of the population of England for the seventeen years 1838–1854. This observation was published in the year 1864, by authority of the Registrar-General for England, and was accompanied by the "English Life Table" deduced therefrom by Dr. William Farr.

In the earlier part of the English observation, made for the seven years ending with 1844, and published in 1849, the numbers of the living and dying, according to age, were given for quinquennial intervals at all ages above 15 years. But in the observation for the total period of seventeen years ending with 1854 the numbers living and dying, according to age, are given for decennial intervals only at ages above 15 years. No reason has been assigned for thus withholding information which is very valuable as an index of the truth, or want of truth, in the reported ages and numbers of the living and dying on which the "English Life Table" is founded. On the present occasion, however, this defect in the English observation for the entire period of seventeen years has been remedied, as may be seen on reference to Tables IV. and V. hereunto annexed. The rates of mortality for decennial intervals of age have been given for the period of seven years and for the period of seventeen years, whilst the rates for quinquennial intervals of age have been given also for the seven years ending with 1844. From these data the quinquennial rates for the seventeen years ending with 1854 have been determined as nearly as can be desired for any useful purpose.

All Tables of mortality, especially those founded on good observations, agree with one another in exhibiting one uniform progressive rate of *increase* of vital force according to age *during childhood*, and another uniform progressive rate of *decrease* according to age *during manhood*, reckoning from puberty to the latest age of life. The true law according to which the vital force uni-

formly increases during childhood, as well as the true law according to which the vital force uniformly decreases during manhood, were first communicated to the public through the Philosophical Magazine of January 1866, in a paper written by me. I had previously, in the year 1832, given to the public a triple series of "Life Tables," all founded upon an empirical law which yields results nearly coincident with the results of the true law published in 1866. For practical purposes, in the construction of Tables of mortality, it is not easy to determine whether the true law of 1866 ought to be preferred to the empirical law of 1832. In either case the law of variation of vital force from birth to the end of life is expressible in very simple terms, the result in either case being a differential of the logarithm of the living ($d \,.\, \log_e P$) of great simplicity. But when the two differentials are integrated, the resulting formula for the living (or survivors) at any specified age t or $a+t$ is found to be more simple when the empirical law is adopted than when the true law is adopted as the basis of calculation.

In the Philosophical Magazine for January 1866 (No. 206, page 9), it has been shown, according to the true law, that the force of mortality at any age, either in the period of childhood or in the period of manhood, is known when the force of mortality at any other age in the same period is known, from the formula following,

$$\frac{\alpha_t}{\alpha_0} = \left(\frac{a}{a+t}\right)^{\frac{1}{k}},$$

wherein t is the difference of age ; a is a constant representing distance (in time or age) from a fixed point, which is one of the two zeros of life ; α_0 is a given or observed force of mortality at a known absolute age a ; α_t is the force of mortality to be determined for any other absolute age $(a+t)$; and wherein $\frac{1}{k}$ is the hyperbolic logarithm of 10, and equal to 2·302585.

There are two zeros of vital force—one belonging to the period of childhood, and the other to the period of manhood. The zero of childhood is at the age $2\frac{1}{4}$ years *before* birth, or at the age $1\frac{1}{2}$ year before conception. The zero of the period of manhood is at the age 102 years after birth-time. The length of the period of childhood (which terminates at 9 years after birth-time) is $2\frac{1}{4}+9=11\frac{1}{4}$ years. The length of the period of manhood is $102-12=90$ years. The length of the period of manhood is just *eight* times the length of the period of childhood. The *increase* of vital force during each year in childhood is just eight times as great as the *decrease* of vital force during each year in the period of manhood. There is an intermediate period, from

the age of 9 to the age of 12 years, during which the rate of mortality is constant and at a minimum. It may be well to observe that the zero of life in the period of childhood may be real and mark the commencement of animal organization. Also it may be useful to observe that, if the law of mortality is continuous above and below 84 years of age as well as above and below birth-time, it will ensue that the rate of mortality at the age 90 years is equal to the rate of mortality immediately after the time of conception, and the rate of mortality at the age 96 years is equal to the rate of extinction of germs existing at the age of 9 months, measured from the day of commencement of organization.

The differential of the hyperbolic logarithm of the living or surviving at any age $a+t$ is known when the force of mortality α at the absolute age a measured from one of the two zeros of vital force is known, and is of the form following:—

$$d.\log_e P_t = \alpha\left(1+\frac{t}{a}\right)^{-\frac{1}{k}} dt.$$

The above equation yields on integration (after assuming P to be equal to unity when $t=0$) the following equation, corresponding to any absolute age $a+t$,

$$\text{com.}\log P_t = -\frac{k\alpha a}{n}\left\{1-\left(1+\frac{t}{a}\right)^{-n}\right\},$$

wherein $n=\frac{1}{k}-1=1{\cdot}302585$, and wherein α is the decrement in a unit of time on a unit of life, at the absolute age a whence t is measured, the infinitesimal rate of decrement for the same precise age being αdt.

The above formula for the surviving *population* from a given *age* a to any other age $a+t$ is similar to the formula which represents the ratio of increase of the expansive force of *water* (with its steam envelope) from a given *temperature* a to any other temperature $a+t$, measured from the *zero of heat*, which is at 276° C. (or 496°·8 F.) below the temperature of melting ice. That is to say, the law of surviving population according to *age* is the same as the law of expansive force of water according to *temperature*. Both laws are expressed by similar functions of the variables, whether in time or in temperature. The expansion by heat of the force of water (or of steam incumbent on water) is the chief instrument employed by man in producing motion for mechanical purposes. In interest and importance to man, the law of vital force is at least equal to the law of steam force. The knowledge of either of these two laws is as valuable as the knowledge of any other law which concerns mankind.

In the case both of surviving population and of steam force, $d \log_e P$ is of the same form though of different signs, whether P represents *population* or *pressure* per square foot of steam of maximum density. The differential of $\log_e P$ represents decrement in one case and increment in the other case. Surviving population is always diminishing as age increases; whilst steam force is always increasing as temperature increases.

In the case of population, $d \,.\, \log_e P$, or $\frac{dP}{P}$ represents rate of decrement of life or force of mortality at the absolute age $a+t$. In the case of steam force, $d \,.\, \log_e P$ stands for rate of increment of force, to which no specific name is attached We know, however, something of the chief factor $\left(1+\frac{t}{a}\right)^{-\frac{1}{k}}$ contained in the expression $d \,.\, \log_e P$ applicable to the pressure of steam of maximum density; for if steam were a perfectly elastic gas and did not increase in density according as the temperature of the subjacent water increased, in that case the increment per degree of the expansive force of such steam at any temperature $a+t$ would be represented by $\alpha\left(1+\frac{t}{a}\right)^{-1}$, if α represented the increase of expansive force per degree at the temperature a. That is to say, the factor which represents increment of force per degree in the two cases is the same, with this difference, however, that the exponent of the factor in one case is unity and in the other case $\frac{1}{k}=2\cdot302585$. The law just mentioned as expressing the increment per degree of expansive force of a perfect gas according to temperature, was discovered eighty years ago, by Dalton in England, and by Gay-Lussac in France. The quantity a measuring degrees from the zero of heat is the same in the case of air as in the case of steam of maximum density. The value of a is 276° C., being the distance of the zero of heat below the temperature of melting ice.

Recurring to the formula for the force of mortality already given, we have, in the period of childhood, for the force of mortality at any age t measured from birth-time, where α_0 is given by observation and $a=2\cdot25$ years,

$$\alpha_t=\alpha_0\left(\frac{a}{a+t}\right)^{\frac{1}{k}}=\alpha_0 a^{\frac{1}{k}}\left(\frac{1}{a+t}\right)^{\frac{1}{k}}=\mathrm{M}(a+t)^{-\frac{1}{k}}.$$

That is, the force of mortality at any age t varies inversely as $\mathrm{R}^{\frac{1}{k}}$, if R be taken equal to $(a+t)$ and be made to represent distance in time or age from a fixed point which is the zero of

vital force. The chief of physical forces is that of gravity, which, according to distance from a fixed point in space, varies inversely as R^2. That is to say, the law of variation of the force of mortality measured from a central point, differs from the law of variation of the force of gravity similarly measured, only in the exponent of the radial distance. The exponent is *two* in the case of the force of gravity, and the exponent is $\frac{1}{k}$, or 2·302585 in the case of the force of mortality.

The empirical formula published in 1832 was founded on the supposition that the mortality according to age decreases or increases in a constant geometrical ratio in each of three definite periods of human life. The com. logs. of the three constant ratios are $-·17$, $+·0128$, and $+·0333$; the corresponding numbers being ·6761, 1·0299, and 1·0797. The first period begins at birth, and ends near 9 years of age. The second period begins at 12 and ends near 55 years of age. And the third period begins near 55 years of age, and continues until the end of life. There is probably an intermediate fourth period, from the age of 9 to the age of 12 years, during which the rate of mortality is constant and at a minimum.

From the above law of geometric increase or decrease of mortality according to age, was obtained for each of the three periods the differential equation following, viz.

$$d \, . \log_e P_t = -\alpha p^t dt\,;$$

and afterwards by integration, assuming $P=1$ when $t=0$,

$$\text{com. log } P_t = \frac{k^2\alpha}{\lambda p}(1-p^t) \text{ or } P_t = 10^{\frac{k^2\alpha}{\lambda p}(1-p^t)}.$$

The quantity α in the empirical formula of 1832 represents the annual rate of mortality at the precise age a whence t is measured, that is when $t=0$. The actual or infinitesimal rate of mortality at the time or age when $t=0$ is αdt. The actual or infinitesimal rate at any other point of time, say t years or fractions of years, is $\alpha p^t dt$. The quantity α thus used to indicate the rate of mortality at a particular point of age was not known to the public until the year 1832. In my book of "Life Tables," published in that year, the above quantity was first described and made the subject of a special Table, of which the following is the title:—"Table A 27, showing at quinquennial intervals of age the force of mortality, or the number of deaths which would occur in one year, upon 100 constantly living." Without the quantity α, as first described by me, any formula similar to that of com. log $P_t = \frac{k^2\alpha}{\lambda p}(1-p^t)$ is of no use except for the interpola-

tion of new values of P between two or more values of P_t extracted from any Table of mortality not regulated by any definite law of decrement of life according to age.

In facilities afforded for the rapid construction of Tables of mortality, the formula of 1832 has the advantage over the formula of 1866, chiefly through yielding successive values of $\log \Delta \log P_t$ differing from one another by a constant quantity which is the common logarithm of the annual ratio of increase of the mortality according to age. The formula of 1832 yields the equation following,

$$\log \Delta \log P_{t+1} - \log \Delta \log P_t = -\log p.$$

The formula of 1866 yields

$$\log \Delta \log P_{t+1} - \log \Delta \log P_t = -\frac{1}{a+t} \text{ nearly.}$$

In the former case the numbers in the column containing $\log \Delta \log P$ are obtained *with exactitude* by successive additions of a constant which is $\log p$. In the latter case the numbers in the same column are obtained *nearly* by successive additions of the variable $\frac{1}{a+t}$. The smaller the intervals of age adopted, the nearer will be the approach to exactitude in the latter case. For practical purposes, the results from both formulæ, obtained as above, will be equally valuable when the intervals of age are yearly. Nevertheless the above short method of constructing Tables according to the formula of 1866 is not likely to find favour with calculators; for they will generally prefer the direct use of the formula yielding accurate results, to the indirect and short course attended with errors however insignificant.

The vital force *relative to age* is probably the same in all individuals, the rate of increase of such force during childhood and the rate of decrease during manhood being the same for all. But the *absolute* vital forces at the same ages may vary greatly when individuals are compared with individuals and classes with classes. One of the earliest fruits of the study of the law of human mortality was the discovery of the fact that the rates of mortality, at all ages, of the populations of large towns were much greater than the rates, at the same ages, prevailing in the populations of the small towns and villages of the same nation. The general rule appeared to be, that the absolute rates of mortality at every age increased according as the magnitude and density of these town populations increased. The earlier writers on human mortality considered large cities to perform the function of graves, in swallowing up all excess of births over deaths, and thus preventing the populations of long settled countries from increasing.

In the year 1832 the present writer gave to the public three

series of theoretical Life Tables—one representing "Village Mortality," another "Mean Mortality," and the third "City Mortaly," the principal series being that of Mean Mortality. At any given age the rates of mortality in the three Tables are to one another in the proportion of the numbers 5, 6, and $7\frac{1}{2}$ respectively. The same three numbers were intended to represent for the fixed age of ten years the annual mortality per thousand living according to the same three several Tables. The above three Tables were deduced from the same formula,

$$\text{com. log } P_t = \frac{k^2 \alpha}{\lambda p}\,(1 - p^t),$$

with the three different values of α above mentioned. These Tables were the first ever published in which the rate of mortality at any age was connected by a continuous and definite law of increase or decrease with the rate of mortality exhibited for every other age. The first of these theoretical Tables, designated as "Village Mortality," is almost in exact coincidence at every age with Heysham and Milne's Table for Carlisle (published in 1815), as may be seen on inspection of Tables I. and VI. hereunto annexed.

In the 'Lancet' of the 9th and 16th of March, 1850, there appeared a paper in which I compared the results of the "Village," "Mean," and "City" Tables of mortality with the observed rates of mortality, according to age, of various parts of the population of England during the seven years 1838–1844, these observed rates having been published by authority of the Registrar-General in the year 1849. Extracts from these compared results will be found in Table III. hereunto annexed. On inspection of this Table it will be seen that the mortality, according to age, of the total male population of the four healthiest of the eleven Registrar's districts into which England has been divided is sufficiently well represented by the theoretical Table of "Village Mortality." Also it will be seen that the theoretical Table of "City Mortality" is a good representation of the mortality, according to age, of the male population of the chief towns of England. Taking four classes of such towns, arranged according to intensity of mortality, it will be seen that the mortality according to the "City" Table, at the various intervals of age, agrees nearly with the mean mortality observed in these four classes of chief towns.

It is worthy of remark that, although the "City" Table is a good representation of the mortality of the population of English cities at ages under 10 years and at ages above 30 years, it is not so for the intermediate period of age. One of the remarkable results of the English observation is, that the mortality of the populations of great towns between the ages of 10 and 30

years is shown to differ very little from that of the general population at the same interval of age. If the fact is in accordance with the observation, the result may be ascribed to the free interchange of town and country population at this interval of age. There commonly occurs at this interval of age a great influx of population into the large towns from the surrounding country. A portion of these immigrants become a part of the permanent population of these large towns; another portion returns and again form part of the country population. The greater the competition and the greater the freedom of interchange of the country with the town population, the more will the mortality of the two classes of population, between the ages of 10 and 30 years, approach to equality.

The English Life Table for males, which was published in 1864, coincides nearly at all ages with my theoretical Table of "Mean Mortality," published in 1832, and with the Table deduced from my formula of 1866. This will be seen on inspection of Table II. hereunto annexed, in which is exhibited for quinquennial intervals of age, according to the three Tables, the numbers surviving and the numbers dying relative to 1000 survivors at the age 12 years. The differences between the English Life Table and the two theoretical Tables are small; and these differences are of no importance, because they are equalled, if not exceeded, by the errors of observation and errors of calculation involved in the English Life Table.

In Table IV. (hereunto annexed) a comparison is made for *decennial* intervals of age, from the age 15 to the age 95 years, of the observed rates of mortality of the total male population of England for the seventeen years 1838–1854, with the corresponding rates exhibited by the English Life Table. In three out of the eight decennial intervals of age compared, there is a considerable discrepancy between the rates observed and the rates exhibited by the English Life Table. The three errors of calculation are all in the same direction, and are in diminution of the rates of mortality observed. The error at the decennial interval of age 15 to 25 is 8·5 per cent., at the interval from 75 to 85 it is 7·4 per cent., and at the interval from 85 to 95 years it is 13·1 per cent.

In Table V. (hereunto annexed) a comparison is made for *quinquennial* intervals of age, from the age 25 to the age 75 years, of the observed rates of mortality of the total male population of England for the seventeen years 1838–1854, with the corresponding rates exhibited at the same ages by the English Life Table. In these ten quinquennial intervals of age the proportional errors of observation vary from 6 per cent. to 12 per cent., and are alternately positive and negative. The mean quinquennial error of observation is 9 per cent., either positive

or negative. In the construction of the English Life Table it has been assumed that the errors of any two consecutive quinquennial rates observed are in opposite directions and balance one another. This is the most favourable mode of estimating the amount of error existing—one observed rate being supposed to be 9 per cent. *above* the true rate, and the next observed rate being supposed to be 9 per cent. *below* the true rate. The least favourable mode of estimating the amount of error existing is by supposing that the errors in two consecutive quinquennial rates are *not* in opposite directions, and that the errors in such observed rates are united or concentrated on one only of the two observed rates, so that the proportional errors are alternately 18 per cent. and *nothing*.

In observations of rates of mortality according to age, no lower estimate than 5 per cent. can be admitted as the probable amount of errors of observation. This amount or rate of error of observation is fifty times as great as the rate of error of observation made on the force of steam according to temperature. The formula for steam-force according to temperature is the same as the formula for surviving population according to age, with difference of sign only. Either formula is

$$\text{com. log } P_t = \pm \frac{k\alpha a}{n}\left\{1-\left(1+\frac{t}{a}\right)^{-n}\right\}.$$

In the case of steam-force, the formula gives results which seldom differ from the results of observation so much as *one in a thousand* at any temperature from 30° to 230° C., or from 86° to 446° F. The observations on steam-force alluded to are those of M. Regnault, published in 1847, and contained in the twenty-first volume of the *Mémoires de l'Académie de l'Institut de France*, p. 624. This insignificant amount of error of observation is applicable not only to the quantity P representing pressure in pounds to the square foot, but also to the rate of increment $\frac{\Delta P}{P}$ for any degree of temperature throughout the above range of 200° C. The comparison between the observed rates and the theoretical rates for steam-force may be seen at pages 185 & 186 of the Philosophical Magazine for March 1865.

In Table VI. (hereunto annexed) is presented a comparison of the results of the two principal observations which have been made in England on the mortality according to age of children below 12 years of age, with the results at the same intervals of age as indicated by two different theoretical Tables. It will be seen that, with exception of the first month from birth, the Carlisle Table of Heysham and Milne is in close agreement with my Table of "Village Mortality" (and with the empirical formula of 1832 on which it is founded) at each of nine intervals of age comprehended between birth and 12 years of age. Also it will be seen that, with a

similar exception, the English Life Table for males is in close agreement, at the same nine intervals of age, with the Table constructed according to the true law and formula of 1866. In the two excepted cases the apparent defects are the same, and consist in the observed rates of mortality in the first month from birth being just three times as great as they ought to be according to either of the two theoretical Tables. This apparent defect will vanish if it be assumed, as is probably true, that two out of three of the deaths in the first month after birth would have been uterine deaths, in the eighth and ninth months of pregnancy, if approaching death had not induced premature birth.

The errors of construction in the "English Life Table" at ages above 75 years (as exhibited in Table IV. hereunto annexed) have their origin in a novel principle adopted for the determination of the annual rates of mortality at the precise ages 20, 30, . . . 80, and 90 years. The constructor of that Table (Dr. Farr) has made the gratuitous assumption that the above annual rates are identical with the annual ratios of the dying to the living according to observation for the decennial intervals of age, 15–25, 25–35, ... 75–85, and 85–95 respectively. This assumption, unsupported by any evidence, although near the truth at ages under 55 years, is probably more or less erroneous at every age. At ages above 75 years there is no appearance of truth in the above assumption, as may be seen on inspection of any ordinary Table of mortality. For example, according to my Table of "Mean Mortality," the annual ratio of the dying to the living for the decennial interval from 75 to 85 years of age is identical with the annual rate of mortality at the precise age 79·1 years instead of 80 years. Also the annual ratio of the dying to the living in the decennial interval from 85 to 95 years of age is identical with the annual rate of mortality at the precise age 87·9 years, instead of 90 years, as assumed by Dr. Farr. These differences in age correspond to errors of 7 per cent. and 13 per cent. in understatement of the rates of mortality observed and truly belonging to the ages 80 and 90 years respectively.

The constructor of the English Life Table does not proceed directly to the interpolation of the values of m at annual intervals from the erroneous values of m at decennial intervals assumed as above, but commences by deducing for each of the decennial values of m the corresponding value of the probability of living one year by the use of my formula com. $\log \mathrm{P}_t = \frac{k^2\alpha}{\lambda p}(1-p^t)$, and making t = unity. The probabilities of living one year at the precise ages 20, 30, ... 80, and 90 years having been thus erroneously obtained, the corresponding probabilities of living one year for all intermediate years of age have been interpolated by the method of finite differences.

In applying my formula to the determination of the probability of living one year at the precise ages 20, 30, ... 80, and 90 years, Dr. Farr substitutes the ambiguous quantity m for the α of my formula, at the same time* describing m as equal to the annual rates of mortality at the "*precise ages*" 20, 30, . . . 80, and 90 years. According to this definition, the m of Dr. Farr's formula is identical with the α of my formula as described by me in 1832. But this definition is immediately followed by the inconsistent and contradictory statement that m at the age 20 years is represented by the mortality "*ruling*" from the age $19\frac{1}{2}$ to $20\frac{1}{2}$ years. This statement is elsewhere confirmed by Dr. Farr, and made to extend to quinquennial and to decennial intervals of age. The erroneous principle adopted in the construction of the English Life Table is, that the annual rate of mortality at the middle point of any interval of age is identical with the annual ratio of the dying to the living during that interval, whether such interval is one year, five years, or ten years. This erroneous principle may otherwise be described as resting on the erroneous and gratuitous assumption, that the area of the curve of surviving population, or $\int \mathrm{P}dt$, between limits t and $t+1$ (in age) is always represented by the ordinate (multiplied by unity) corresponding to the abscissa $t+\frac{1}{2}$, whatever be the unit of age, whether one year, five years, or ten years.

In its second and principal signification, as adopted by Dr. Farr, the apparently simple quantity m, which has been substituted for the constant quantity α of my formula, is in reality a variable quantity of great complexity, and more unknown than the quantity P, which is to be expressed in terms of m. For m, which represents the "*mean mortality*" at any or the $(t+1)$th interval of age, is of the form following:

$$m=\frac{\mathrm{P}_{t+1}-\mathrm{P}_t}{\int \mathrm{P}dt}.$$

The numerator of the above fraction, expressed in terms of the variable t and constants, is

$$10^{\frac{k^2\alpha}{\lambda p}(1-p^{t+1})}-10^{\frac{k^2\alpha}{\lambda p}(1-p^t)}$$

whilst the denominator for integration between limits t and $t+1$ is

$$\int 10^{\frac{k^2\alpha}{\lambda p}(1-p^t)}dt.$$

If the above fraction could be expressed in finite terms, there would be no ground for supposing that the value of m for the first interval of age, if multiplied by p^t, would represent the value of m for the $(t+1)$th interval of age. No more is there any ground for supposing that the differential of $\log_e \mathrm{P}$ is equal

* See Introduction to 'English Life Table,' pp. xxiii & xxiv.

to $-mp^t dt$, as is alleged by Dr. Farr. Even if this had been the true differential, the integral thereof could not have been at all similar to my formula (of 1832), which is derived from the differential $-\alpha p^t dt$; for m is a function of t, whilst α is a constant.

TABLE I.—Proportional numbers Living or Surviving at decennial intervals of age, according to three theoretical Tables of Mortality, compared with similar numbers exhibited by three well-known Tables of Mortality not supposed to be regulated by any definable law according to age.

Age in years.	Heysham and Milne. Carlisle (1815).	Edmonds's "Village Mortality" (1832).	Milne's Sweden. Males to 1795.	Edmonds's "Mean Mortality" (1832).	Halley's Breslau (1693).	Edmonds's "City Mortality" (1832).
0	1562	1514	1642	1465	1916	1611
5	1062	1063	1089	1064	1133	1080
10	1009	1010	1015	1013	1023	1016
12	1000	1000	1000	1000	1000	1000
15	984	983	981	980	972	975
25	919	919	906	904	888	881
35	838	840	810	811	759	770
45	739	744	702	701	615	641
55	636	632	566	576	452	502
65	472	476	390	410	297	328
75	262	259	174	197	136	132
85	70	70	34	41	23	18
90	22	22	10	10	2	3
95	5	4	2	1		

TABLE II.—Comparison of the numbers Surviving at successive quinquennial intervals of age, according to the "English Life Table" for Males, with similar numbers from two theoretical Tables; the common basis adopted being 1000 Living or Surviving at the age 12 years.

Age.	Edmonds's "Mean Mortality" (1832).		English Life Table. Males (1864).		Edmonds's formula of 1866.	
	Living.	Dying in 5 years.	Living.	Dying in 5 years.	Living.	Dying in 5 years.
0	1465	401	1465	405	1427	372
5	1064	51	1060	49	1055	44
10	1013	33	1011	25	1011	28
15	980	36	986	31	983	31
20	944	40	955	40	952	34
25	904	44	915	43	918	38
30	860	49	872	45	880	43
35	811	53	827	48	837	48
40	758	57	779	53	789	55
45	701	61	726	58	734	61
50	640	64	668	68	673	70
55	576	74	600	78	603	79
60	502	92	522	90	524	88
65	410	105	432	105	436	97
70	305	108	327	110	339	103
75	197	93	217	99	236	101
80	104	63	118	70	135	83
85	41	31	48	34	52	44
90	10	9	14	12	8	8
95	1	1	2	2	0	0

TABLE III.—Annual Mortality per cent., according to age, during the seven years 1838–44, of the Male Population of the chief Towns of England, and of the four healthiest Registrar's Divisions of England, compared with the theoretical Tables of "City Mortality" and "Village Mortality," published in 1832.

(From the 'Lancet,' vol. i. (1850) p. 330.)

Interval of age.	Registrar's divisions, Nos. 2, 4, 5, & 11.	Edmonds's "Village Mortality."	Twelve large towns.	Eight larger towns.	Edmonds's "City Mortality."	London.	Liverpool and Manchester.
0–5	5·57	7·45	8·46	10·51	8·47	9·31	14·01
5–10	·85	1·02	1·14	1·24	1·24	1·24	1·58
10–15	·18	·54	·50	·57	·82	·48	·60
15–25	·81	·67	·86	·89	1·01	·76	·96
25–35	·95	·90	1·08	1·12	1·35	1·07	1·28
35–45	1·07	1·21	1·43	1·62	1·81	1·79	2·07
45–55	1·48	1·62	2·06	2·42	2·43	2·73	3·20
55–65	2·65	2·78	3·55	4·26	4·14	4·81	5·27
65–75	5·83	5·87	7·06	8·47	8·64	9·18	10·39
75–85	13·22	12·11	15·58	16·99	17·50	18·47	20·24
85–95	28·92	24·05	......	......	34·10	32·00	
All ages ...	2·02	......	2·57	2·91	......	2·74	3·51
Population in thousands ...	2619	......	256	283	......	913	202

TABLE IV.—Annual Mortality per cent., according to age, of the total Male Population of England during the 17 years 1838–54, according to observation, and according to the "English Life Table" intended to represent the result of such observation.

Interval of age.	Edmonds's "Mean Mortality" (1832).	Observed rate, 7 years (1838–44).	Observed rate, 17 years (1838–54).	English Life Table for 17 years.	Difference or error.	Proportional error per cent.
0–5	6·70	7·07	7·25	7·01		
5–10	·99	·93	·92	·96		
10–15	·65	·50	·52	·50		
15–25	·81	·80	·82	·75	−0·07	8·5
25–35	1·08	·97	1·00	1·00		
35–45	1·45	1·25	1·28	1·29		
45–55	1·95	1·78	1·85	1·90		
55–65	3·33	3·14	3·18	3·24		
65–75	6·99	6·61	6·69	6·58		
75–85	14·31	14·39	14·76	13·74	−1·02	7·4
85–95	28·17	29·65	30·14	26·20	−3·94	13·1
All ages...	2·55	2·27	2·33	2·50		

TABLE V.—Showing for quinquennial intervals of age, above 15 years, for the total Male Population of England, the discrepancies between the rates of mortality observed and the rates exhibited by the "English Life Table," published in 1864.

Interval of age.	Sweden, Males. 20 years to 1795.	Edmonds's "Mean Mortality" (1832).	Observed rate, 7 years to 1844.	Probable rate, 17 years to 1854.	English Life Table for males.	Difference or error.	Proportional error per cent.
	per cent.	per cent.	per cent.	per cent.	per cent.		
15–20	·68	·75	·71	·73	·63	− ·10	13·7
20–25	·90	·87	·92	·94	·87	− ·07	7·4
25–30	1·06	1·00	·98	1·01	·96	− ·05	5·0
30–35	1·17	1·16	·97	1·00	1·06	+ ·06	6·0
35–40	1·26	1·35	1·26	1·29	1·20	− ·09	7·0
40–45	1·60	1·56	1·25	1·28	1·40	+ ·12	9·4
45–50	1·92	1·81	1·73	1·80	1·68	− ·12	6·7
50–55	2·40	2·10	1·84	1·91	2·14	+ ·23	12·0
55–60	3·00	2·74	2·97	3·01	2·77	− ·24	8·0
60–65	4·39	4·02	3·32	3·36	3·78	+ ·42	12·5
65–70	6·63	5·88	5·97	6·05	5·47	− ·58	9·6
70–75	9·28	8·58	7·41	7·49	8·12	+ ·63	8·4
75–80	13·25	12·50	12·71	12·87	12·00	− ·87	6·8
80–85	18·64	18·16	17·53	17·75	17·34	− ·41	2·3
85–90	24·67	26·23	28·33	28·55	24·46	−4·09	14·3
90–95	33·52	37·61	35·51	35·79	33·67	−2·12	4·7

TABLE VI.—Proportional numbers Dying at each of nine intervals of age below 12 years, relatively to 1000 Survivors to that age, according to the Carlisle Table of Heysham and Milne, according to the "English Life Table," and according to each of two theoretical Tables of Mortality.

Interval of age.	Heysham and Milne. Carlisle (1815).	Edmonds's "Village Mortality" (1832).	English Life Table. Males (1864).	Edmonds's formula of 1866.
0 to 1 month.	83	20	77	25
1 „ 3 months.	38	37	46	44
3 „ 6 „	40	50	46	53
6 „ 12 „	80	82	71	73
1 „ 2 years.	107	114	78	84
2 „ 4 „	122	118	67	74
4 „ 6 „	50	50	34	33
6 „ 9 „	29	28	29	24
9 „ 12 „	11	15	17	17
Total deaths under 12 years	560	514	465	427

IV. *Fundamental Principles of Molecular Physics. Reply to* Professor Bayma. *By* Professor W. A. NORTON.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE paper by Professor Bayma, entitled "Fundamental Principles of Molecular Physics," published in recent Numbers of the Philosophical Magazine, is obviously of a character to demand some answer at my hands. In replying to it I do not propose to take up in detail, and in the order in which they occur, all the points made by the learned author, nor strive to make good all the positions before taken in my reply to his criticisms on my 'Memoir on Molecular Physics.' My aim will be to present the important points on which we are at issue in what appears to me to be their true attitude, in such order as may best conduce to a clear understanding of the whole subject, alluding occasionally to such side issues as may demand attention. The cause of truth will apparently be best subserved in this way; and this is of far more importance than that my accuracy and consistency should be formally justified by defending anew every position I have taken. Whether any important position, either taken in my original paper or in my reply to Professor Bayma's criticisms, has been effectually assailed or not, there will be a fair opportunity of judging when the whole ground shall have been gone over.

By way of introduction to a general view of the case, I will first remark that I did not mean to convey the idea, in what Professor Bayma calls my first proposition, that molecular science is "without established principles," is a "pure heap of hypotheses." I had no thought of implying that I did not regard the existence of matter, with its fundamental properties of inertia, &c., the operation of forces of attraction and repulsion in nature, and other kindred principles, as established truths; and it is surprising that such an intimation should have been thrown out by my critic, who, with all his unquestionable acuteness, is, I doubt not, animated by a sincere desire to deal justly and with entire fairness. I meant, and could reasonably be supposed to mean, no more than that every new theory of molecular physics must of necessity *involve one or more hypotheses* that "have been rendered more or less probable, either by induction from observations or *à priori* reasonings," and to be tested by a comparison of the deductions from the theory with facts, and therefore that its foundation is essentially hypothetical—just as it is affirmed that the strength of a structure is the strength of its weakest part. The doctrine is, in other words, that a new theory of molecular physics must, when first pro-

pounded, occupy precisely the same hypothetical position that all former physical theories have at first done—as that of universal gravitation, the undulatory theory of light, &c. It is by triumphantly withstanding all possible tests that these and other theories have come to be admitted among the established truths of physical science. It is in this way alone that physical science has hitherto made all its great advances. In no instance has a physical theory sprung into existence, Minerva-like, in full armed panoply, the complete full-grown impersonation of wisdom and truth.

It does not follow, then, as our author intimates, because such theories have had, and as I conceive must continue in each new instance to have, more or less of a hypothetical foundation, that no physical theory can lead to established truths. The deductions from it have, it is true, no higher certainty, as mere deductions, than the fundamental induction from which they are derived; but every legitimate deduction that accords with known facts, furnishes thereby a new confirmation of the essential truth of the theory. It gains assurance of strength by its victories, and, when crowned with years of triumph, is worthy of all honour, despite its humble origin.

Professor Bayma conceives that the time has arrived when a theory of molecular physics can be securely erected upon a few philosophical principles which may be regarded as established truths, and that the legitimate deductions from the theory will have the same character of certainty. If this claim could be admitted, I should be far from desiring to put a single straw in the way of his success, and would gladly recognize the "eternal verities" evolved from his philosophy. Nor would there be of necessity any conflict between us; for in proportion to the strength of my confidence in the essential truth of my own theory of the modes of evolution of phenomena, would be the strength of my conviction that his theory must embrace my own generalizations within its comprehensive grasp, though placing them in a new attitude and on a deeper foundation. But I cannot but entertain a decided conviction that our author's claim, that his legitimate theoretical deductions are positive certainties, rests on fallacious grounds. It implies that his fundamental principles, whether formally expressed or implied, are all either universally admitted truths, or truths which he has himself demonstrated. Now certain of these principles do not, in the nature of things, admit of positive proof. They cannot have any other foundation than certain conceptions with regard to matter or active powers which can only be regarded as mere assumptions. For example, it is laid down as a fundamental principle that matter in its ultimate analysis is made up of absolute

points separated by finite distances, every one of which acts upon every other point, and hence that there can be no such thing in Nature as an atom that has continuous extension. Now this principle is no inevitable deduction from recognized facts; for the only certain knowledge furnished by the entire range of physical science with regard to the so-called atoms, is that they have certain properties and active powers. The essential origin and mode of evolution of these properties and powers must for ever remain an impenetrable mystery. It may be confidently asserted that few links of the mystic chain that binds each ultimate atom to the throne of the Creator will ever be certainly discerned. We may indeed recognize that the so-called "chemical atoms" are really complex in their constitution, and should accordingly be termed "primitive molecules," as both Professor Bayma and myself maintain, and frame hypotheses as to the nature of their physical constitution and the immediate origin of the forces they exert, suggested by physical phenomena, and to be tested by comparing the deductions from them with facts; but the elements, or primary atoms, of which they are composed, what are they? Are these of necessity mere points, mere mathematical centres of force? Is it not absurd to suppose that when we can know nothing of the essential nature and origin of the primary powers, or activities, of these atoms, anything can be predicated with certainty with regard to their size and the question of their continuity or non-continuity, and to claim that a certain conception formed of their geometrical character is not an assumption, not an hypothesis, but an absolute verity. Our author's "demonstration," that an atom having continuous extension is an impossibility, rests upon the assumption that if an atom be conceived to be continuous, each point of it must act upon every other point in the same manner and in the same degree at equal distances. Now in our absolute ignorance of the manner in which force and matter are linked together, how can we be sure that this is an inevitable conclusion. It is in fact a mere inference from the assumption that force may be evolved from a mathematical point, and take effect upon another mathematical point which is the centre of a similar activity. If this be a truth, the knowledge of it can be gained from inspiration alone.

Let us examine it a little from a philosophical point of view, somewhat different from that which our author occupies. The principle of activity cannot subsist in a mere mathematical point, for activity implies a something to act, and a mathematical point is nothing but position. Also a mathematical point cannot be acted upon, for an activity exerted implies something having receptivity, and a mathematical point can have no such pro-

perty, since it is nothing but position. If it be urged in reply that the points supposed are not mere mathematical points, but also centres of force, the answer is, if the possibility of mere centres of emanation of force be admitted, still to suppose that one centre of force acts upon another is to suppose that one force acts directly upon another force, or that the principle of activity acts upon itself. Again, mobility cannot be predicated of a point, since a force cannot impart motion to nothing, nor to another force or collection of forces in a point. This reasoning may not be deemed conclusive; but the real question here is, not whether it is conclusive or not, but whether it is not as much entitled to be called so as the "demonstration" we find on page 28 of the 'Molecular Mechanics,' that "the hypothesis that bodies are made up of particles materially continuous leads to an absolute impossibility of communication of motion," or as the demonstration on page 30, that "matter cannot be continuous."

If it should be urged that we cannot conceive of an atom of which every point does not possess the same activity as every other point, or that the entire space occupied by an atom should alone determine the definite power which it exerts outwardly and receives, it is equally impossible to conceive of mere points endued with all the essential properties and powers that belong to matter (these powers differing in intensity and kind, although belonging to mere points), resisting change of place with varying degrees of inertia, and retaining the same activities as they shift their position from one point of space to another. We may as well frankly admit that in all such attempts to reach true conceptions we are vainly striving to sound the fathomless depths of the unknown.

Another of Professor Bayma's fundamental principles is, that simple elements act at all distances according to the inverse ratio of the squares of the distance. This principle may be admitted as the law of elementary action if we regard such action as a propagated emanation; and it may be adopted as an hypothesis if we conceive, with Professor Bayma, that such action is instantaneous at all distances; but he undertakes to demonstrate its truth by both "metaphysical and mathematical reasoning." The demonstration, whatever may be said of the metaphysics, is open to this fatal objection—that it involves the conception that *gravitation and molecular attraction are but the same elementary forces operating at different distances.* To show that *this cannot be true,* let us suppose a primitive molecule posited at the distance (d) from a certain point of the earth's surface, at which the attraction of adhesion becomes sensible; and let us conceive the earth's surface to be perfectly smooth and spherical. Now New-

ton has shown that if the law of elementary action be that of the inverse squares, the attraction of such a homogeneous sphere for an element exterior to it is the same as if the whole mass were concentrated at the centre. The demonstration involves the supposition that equal portions, however small, of each spherical layer are occupied by equal quantities of matter. The principle demonstrated holds good for every distance of the element attracted from the surface—except that at very minute distances, not many times greater than the distance between two contiguous molecules of the earth's mass, it may happen that two lines diverging from the element in question under a small angle will not actually contain within them any matter on the immediately contiguous portion of the earth's surface, and as a consequence the entire attraction of the first spherical layer would be represented by that of its mass concentrated at a point slightly more remote than the centre. The result would then be that, in the case supposed, the entire attraction exerted by the earth would be slightly less than the Newtonian deduction. It follows, therefore, that if the element at the supposed minute distance (d) from the earth's surface were to approach the surface, the entire attraction it would experience would not be sensibly greater, would in fact be less than at the distance (d); whereas the attraction of adhesion that would actually come into play is immensely greater than the simple force of gravity near the surface. We thus demonstrate that *the attraction of gravitation cannot be the force of molecular attraction operating at greater distances,* either as a whole or in its elements; and accordingly show that the law of inverse squares proved for gravitation cannot be extended inferentially, or by any process of reasoning, to the force of elementary attraction at minute distances.

The same important conclusion may be reached more directly in another way. The enormous excess of the attraction of adhesion or of cohesion at distances a little greater than the distance between contiguous molecules, over the force of gravity at the distance (d) above specified, can only be attributed, from Professor Bayma's point of view, to a greatly increased attraction of the molecules lying at or near the earth's surface. Now the number of separate lines that can be drawn from the element attracted through attractive elements so situated is incalculably small, we may say insensibly small, in comparison with the number that can be drawn through more remote elements which by their united action determine the force of gravity; and hence the attraction of adhesion should be incalculably small in comparison with the force of gravity.

It may here be incidentally remarked that, unless the position just taken can be proved to be untenable, it must be admitted

that Professor Bayma's theory not only fails to include the known force of gravitation, but actually excludes it as something altogether impossible—since his supposed or "proved" molecular actions are all that possibly exist in accordance with his fundamental principles, and these, as we have just seen, do not include the actual force of gravity, but have, as their necessary concomitant, an attractive action at considerable distances vastly greater than the actual attraction. Or, if he prefers the other horn of the dilemma and admits the actual force of gravitation, we are then conducted to the inevitable inference that his theory makes no adequate provision for the known molecular attraction, since the molecular attraction deduced from the force of gravity is of an exceedingly small intensity in comparison with the attractive action known to exist.

The same inference may be extended to the force molecular repulsion, since the actual repulsion is in equilibrium with the attraction at ordinary molecular distances; and hence the theoretical repulsion must have an intensity correspondent to that of the theoretical attraction, and therefore be exceedingly small as compared with the actual repulsion. In fact, if I mistake not, the objection here urged saps the foundation of the whole theory developed and maintained with such signal ability by Professor Bayma in his 'Molecular Mechanics.' To comprehend the full force of this objection, it should be borne in mind that our author maintains that all material elements are mere points, and are either attractive under all circumstances or repulsive under all circumstances,—that the action of each element takes effect upon all other elements according to the law of the inverse squares, and without the least interception by intervening elements,—and that these direct actions of the two classes of elements, attractive and repulsive, are the sole determining cause of all material phenomena. It should be added that each "primitive molecule" is conceived to consist of a central attractive portion, and an exterior repulsive envelope (each of these being composed of elements separated by finite distances)—and that the "molecular radii" are regarded as "infinitesimal quantities," in comparison with the distance between contiguous molecules at which their effective attraction manifests itself.

We find in the 'Molecular Mechanics' the following fundamental propositions: "one and the same element A cannot attract the element B and repel another element C when B and C are equally distant from A;" and "one and the same element of matter cannot be attractive for one distance and repulsive for another." These are not direct inferences from physical facts, since we recognize among molecular actions precisely the differences which it is here stated cannot have place in the activities

exerted by the ultimate elements. But the attempt is made to establish them by metaphysical reasoning, of which it may be said that it involves certain conceptions of the "principle of activity," "nature," and "determinations" of elements, designated as "substance," though they are nothing but mathematical points, which are neither self-evident truths nor have any character of certainty, but are mere shadows dimly discerned in that metaphysical region which the finite mind strives in vain to enter. The most that can be conceded is that they have a certain air of probability, and may reasonably be adopted by our author as hypotheses to be ultimately substantiated or overthrown by the appeal to facts.

It will be apparent from what has been stated that an important difference obtains in the nature of the foundations on which Professor Bayma's theory and my own have been erected, in the methods of construction employed, and in the claims asserted with reference to the true character of the results achieved. The theory developed in my memoir on Molecular Physics rests upon the most comprehensive generalizations and principles to which the progress of physical science has conducted, and in no degree upon metaphysical conceptions or reasonings with respect to the nature of matter, the size of atoms, the possibilities or impossibilities of certain inherent material actions, &c. On the other hand, in the groundwork of Professor Bayma's theory are included, as we have seen, certain conceptions and reasonings of this character which I maintain are fundamentally hypothetical. Professor Bayma has proceeded on the philosophical and what he deems the strictly scientific plan of construction, while I have restricted myself to the simple deduction of molecular forces and phenomena. He claims that his fundamental principles are either universally admitted or demonstrated truths, and that his legitimate deductions are to be received as established truths. I do not venture to prefer any higher claim than that the fundamental principles I have adopted are universally admitted (with the single exception of the hypothesis of an electric fluid or æther; and this is the only distinct fundamental conception which the process of inductive research has evolved from electric phenomena), and that the recognized molecular forces and the various classes of physical phenomena can be legitimately deduced from the few fundamental postulates laid down without the aid of new hypotheses. In this I claim to have pursued the ordinary method of physical speculation, and the only one which has hitherto achieved any substantial success. Professor Bayma virtually admits (Phil. Mag. March 1869, p. 183) that his method is radically different from the methods of research hitherto employed by physicists. This, which he esteems its most excellent feature,

and as constituting an especial claim to favourable regard, will be likely to prove its sufficient condemnation.

The entirely different stand-point occupied by Professor Bayma from that which I have taken, and the consequent liability he has incurred of misunderstanding my views, is the occasion of much of the criticism he has indulged in. Thus he assails from all points, and in a variety of modes, what he regards as one of my strongholds, viz. that a primary atom has continuous extension and is spherical in form. Now, as a matter of fact, in framing my theory I took scarcely any thought of the question of the continuity of matter in a primary atom. Conceiving the real constitution of the atom to be incapable of detection, I simply adopted the ordinary conception of it, recognizing in it the embodiment of three essential truths, viz. (1) that the ultimate element, called an atom, is incapable of division by either mechanical or chemical means, (2) that it acts with equal energy in all directions, (3) that its surface opposes a repulsive resistance to any other atoms that may be urged toward it by the attraction of the whole atom. These three features cannot be conceived to belong to a single point, but may either to a continuous material sphere, or to a spherical collection of material points. It matters not, from my theoretical stand-point, which of these two views be taken.

But I have since been led (see my answer to Professor Bayma's criticisms in the Philosophical Magazine, February 1869, p. 106) to adopt the fundamental conception that the effective attraction of a primary atom of ordinary matter for the luminiferous æther probably consists in a diminished repulsion. Upon this view the question of the size and constitution of primary atoms can have no value in physical science, and may be left for the entertainment of those who have a predilection for metaphysical speculations.

Before taking up briefly some of the specific points discussed in Professor Bayma's paper, it may be well to say a word in reply to his affirmation that "hypothesis begins only where real science ends." I would ask our learned author if real science had come to an end when Newton conceived the hypothesis of universal gravitation and followed it out to its legitimate consequences—or when Huyghens imagined the existence of luminiferous æther waves, and so laid the foundation of the undulatory theory of light.

Yale College, U.S.,
June 1, 1869.

[To be continued.]

V. *Note on the Hydrodynamical Theory of Magnetism.* *By* Professor Challis, *M.A., F.R.S., F.R.A.S.**

IN the Numbers of the Philosophical Magazine for January and February 1861 I proposed a theory of magnetism founded on hydrodynamical principles, which is also reproduced, with modifications and additions, in my work 'On the Principles of Mathematics and Physics,' recently published. It has since occurred to me that an objection might be raised against the theory because it does not account for the variation of magnetic action according to the law of the inverse square, which seems to be established by Gauss's process for determining the absolute measure of the intensity of terrestrial magnetism. The purpose of this Note is to meet this objection.

Whatever may be thought of Gauss's fundamental hypotheses of two fluids acting attractively and repulsively under certain conditions according to the law of the inverse square, and of the dependence of sensible magnetic action on the "separation" of these fluids, it is certain from the numerical results he has obtained that his investigations must have a real physical basis. A true theory of magnetism ought to be capable of indicating what that basis is, and how far the hypotheses are expressions of facts, or are simply empirical. I proceed to try the hydrodynamical theory by this test.

It will be necessary, first, to state the leading principles of this theory. All visible and tangible substances are supposed to consist of inert spherical atoms of constant form and magnitude, retained in positions of equilibrium by the resultant actions of the forces which I have named atomic repulsion and molecular attraction. The laws of these forces admit of being mathematically deduced from the hypothesis of a universal and continuous æther, supposed to press proportionally to its density, and from the combination of its action with the reaction of the atoms due to their constancy of form. The space occupied by atoms is assumed to be very small compared to the intervening spaces, even for substances of great density. This assumption is justified by an inference from the undulatory theory of light, as is shown in page 410 of the above-mentioned work.

These hypotheses being understood, we may next consider what will take place when a steady stream of the æther enters into a substance atomically constituted in the manner above stated. For the sake of precision it will be supposed that the body has the form of a cylinder the diameter of which is small compared to the length of the axis, and that the direction of the axis coincides with that of the stream. Then from the hydro-

* Communicated by the Author.

dynamics of steady motion it follows that the fluid will have greater velocity and less density within the cylinder than without, simply because of the contraction of channel by the occupation of space by the atoms. There will be confluence of the lines of motion towards the extremity at which the stream enters, and equal divergence of the lines of motion from the extremity out of which it issues. These lines, as well as the velocity and density along them, will be symmetrically disposed about the axis of the cylinder prolonged in both directions, and also with respect to a plane transverse to the axis through its middle point. Under these circumstances there is no acceleration of the mean current, the quantity of fluid which crosses any unlimited plane transverse to the axis being the same as if the stream had not been interrupted by the cylinder.

The above description of the courses of the lines of motion applies to any solid cylinder whether or not it be magnetic. If it is not magnetized, but susceptible of magnetism, the modification which the original stream undergoes by passage through the cylinder is proper for magnetizing it. For it is evident that, by reason of the variation of the density of the æther from point to point, the atoms of the cylinder, especially those at and near its extremities, will be caused to vibrate; and it appears from experiment that the magnetizing of a substance is effected whenever a magnetic stream traverses it while its particles are in a state of vibration. This is remarkably indicated by the well-known experiment in which a plate of iron, placed with its faces in the direction of magnetic dip, is magnetized by being repeatedly struck with a hammer. Possibly the permanent magnetism of the loadstone may have been gradually induced by the ætherial streams which relatively pass through it in consequence of the earth's motion in space.

Supposing that the cylinder, either by the process above mentioned, or by some other, has been magnetized, let us inquire what influence this circumstance will have on the stream which traverses it. But it is first necessary to define the magnetized state. According to the theory of magnetism I long since proposed, this state depends solely on a small and regular increment of atomic density from one end to the other of the cylinder, the equilibrium of the atoms being maintained by the equality, at each point, of the atomic repulsion towards the rarer part, and the molecular attraction towards the denser part. Conceive now the ætherial stream to traverse the cylinder in *any* direction. At exit and entrance there will be the same cause of disturbance of the lines of motion as in the previous case of a cylinder of uniform density; and, besides, the gradation of density will have the effect of generating new streams, which for distinction I shall

call *secondary streams*. The particular mode of generation of these streams is next to be considered.

The incident stream being supposed to have originally the same velocity and density at all points of any section transverse to its direction, it follows, by the laws of steady motion, that after entrance into the cylinder its resulting mean velocity will be greater and mean density less, the greater the atomic density. This is an immediate consequence of the contraction of channel by the atoms. Hence the fluid will be impressed at all points in the interior of the cylinder by a constant accelerative force acting in the direction from the rarer towards the denser end. The consequent effective accelerative force will, by reason of the inertia of the fluid, accelerate a given particle towards a transverse plane through the middle point of the cylinder, and equally retard it after it has passed that plane. Thus there will be a maximum of velocity at the points where the plane is cut transversely by the lines of motion. Also as there can be no transfer of the whole fluid mass, supposed to be of unlimited extent, by means of an accelerative force impressed on a limited portion of it, there will necessarily be *return* currents at different distances from the cylinder, such that the lines of motion of these secondary currents will be reentering. The courses of these lines will be symmetrical with respect to the axis and the above-mentioned transverse plane, and will cross this plane outside the cylinder at right angles. Such is the general character of the secondary streams to which the theory attributes the phenomena of the magnet.

It will be seen that the intensity of the secondary stream is the same whatever be the direction of the primary, so long as the latter is of given intensity. Also it must be admitted that the secondary stream, as generated by the interior gradation of density of a magnetized body, is dynamically far more effective than that modification of the primary stream which was above described as being produced whether or not the body be magnetized; for otherwise magnetic streams would be perceptible in the case of a non-magnetized body. The great intensity of the secondary streams is to be attributed to the efficacy of the impressed accelerative forces by which they are generated, the equation $p=a^2\rho$ showing that, on account of the great magnitude of a^2, the extremely small variation of ρ due to the gradation of density may cause a large change of p. In the subsequent reasoning the above-mentioned small modification of the primary stream is left out of account.

It may be supposed that the whole mass of the fluid in which the secondary streams are generated partakes of the primary motion. In that case, if the primary velocity were impressed in

the opposite direction both on the fluid and the cylinder, the secondary streams would be unaffected, the fluid would be reduced to rest, and the cylinder would be made to move in it in a given direction with a given velocity. This is the case of nature, a magnetized body being carried through space by the earth's motion, and its magnetism being the result of the generation of secondary streams by the relative motion of the æther and by the interior gradation of density. It is, however, to be observed that the motion which the earth has in common with the solar system, the motion in its orbit, and the rotation about its axis, produce independent magnetic effects, and that the total magnetism is the *sum* of the magnetisms which these motions would produce separately. The reasons for this statement are that the *resultant* of these motions is not a uniform motion in a fixed direction, and, as there will be occasion to show subsequently, the secondary motions which they would generate singly are such steady motions as can *coexist*.

Reverting now to the case of the magnetic streams of the cylindrical magnet, which may be conceived to have a fixed position in space, let C be the middle point of the axis, and let the density increase from the end A to the end B, so that the course of the secondary stream is in the direction from A towards B. According to hydrodynamical principles, there can be, on the whole, no transfer of fluid across *any* plane perpendicular to the direction of the axis, the motions of the fluid within and outside the cylinder being both taken into account. In calculating the velocity of the fluid at any point, the effect of the occupation of space by the atoms will be considered only so far as it produces secondary streams by the gradation of density.

To show how the above-mentioned condition is fulfilled is the object of the following argument. Conceive the axis to be cut perpendicularly by a plane at the distance x from C in the direction towards B, and draw any straight line from C intersecting the plane in P. Let $\mathrm{CP}=r$, the angle $\mathrm{PCB}=\theta$, and, y being an unknown function of x, let $y^2+x^2=\mathrm{R}^2$. Since the motion of the fluid is wholly in planes passing through the axis, the velocity at P may be resolved into U along CP and W perpendicular to this line. It will now be assumed that for any point in the transverse plane, beyond the distance y from the axis,

$$\mathrm{U}=\frac{\mathrm{VR}^3}{r^3}\cos\theta, \quad \mathrm{W}=-\frac{\mathrm{VR}^3}{2r^3}\sin\theta.$$

The forms of these expressions have been adopted from a consideration of the circumstances of the motion when the fluid is impelled by a moving sphere, in which case, as is known, both V and R are constant, and the expressions apply to all points of

the fluid. We have next to calculate the quantity of fluid which, according to these values of U and W, passes at any instant a given transverse plane.

These velocities, resolved parallel to CB, are respectively $\frac{VR^3}{r^3}\cos^2\theta$ and $-\frac{VR^3}{2r^3}\sin^2\theta$, so that the whole resolved velocity in that direction is

$$\frac{VR^3}{2r^3}(3\cos^2\theta-1).$$

Hence the quantity of fluid which passes the part of the plane exterior to the circle of radius y in the small time δt is

$$\delta t\int 2\pi r\sin\theta\,.\,\frac{VR^3}{2r^3}(3\cos^2\theta-1)d\,.\,r\sin\theta,$$

the integral being taken from $r=R$ to $r=$ infinity. Since $r\cos\theta=x$, this integral is equal to

$$\pi VR^3\delta t\int\frac{dr}{r^2}\left(\frac{3x^2}{r^2}-1\right),$$

which taken between the above limits is

$$-\pi VR^2\left(1-\frac{x^2}{R^2}\right)\delta t.$$

If the plane intersect the axis of the cylinder produced, at any point beyond either A or B, we must suppose that $y=0$, or that $R^2=x^2$. Since in this case the integral vanishes, there is no permanent transfer of fluid across such planes, with respect to which, therefore, the required condition is fulfilled. Thus the assumed expressions for U and W are so far justified.

In other cases, by putting for R^2 the value y^2+x^2, the integral becomes $-\pi Vy^2$. Now let $f(x)$ be the mean velocity with which the fluid within the distance y crosses the same transverse plane in the direction from A towards B, then the whole quantity that passes that plane in the time δt is

$$\pi f(x)y^2\delta t-\pi Vy^2\delta t.$$

Since by the principle already enunciated this quantity is zero, it follows that $f(x)=V$.

Hence, by having regard to the above signification of $f(x)$, and to the circumstance that the lines of motion converge towards the parts about A and diverge from those about B, it is clear that the velocity V *diminishes* with the distance from C according to some unknown law. In default of an exact *à priori* investigation of this law, I shall now make the provisional supposition that V varies inversely as R^3, or that VR^3 is equal to a

constant μ. Then we shall have, at any point exterior to the circle of radius y,

$$U=\frac{\mu}{r^3}\cos\theta, \quad W=-\frac{\mu}{2r^3}\sin\theta.$$

Consequently, at points for which $\theta=0$ and $\theta=\pi$, $W=0$ and $U=\frac{\mu}{r^3}$ reckoned in the direction from A towards B; and at points in the plane through C transverse to the axis, $U=0$ and $W=-\frac{\mu}{2r^3}$. Hence at the same distance r, the *backward* motion across that plane parallel to the axis is *half* the forward motion along the axis; and each of these velocities varies as the cube of the distance from C.

Since y is an unknown disposable quantity, the above supposition that VR^3, or $V(y^2+x^2)^{\frac{3}{2}}$, is equal to a constant, is not illegitimate. The function that y is of x will depend on the form of the magnet. In the case of a cylindrical magnet y will not generally differ much from the radius. It is also to be remarked that the above value of U for a point on the axis, and that of W for a point in the transverse plane, are to be considered as approximative functions of r. The more complete values would probably be of the form

$$U=\frac{\mu}{r^3}\left(1-\frac{h^2}{r^2}\right), \quad W=-\frac{\mu}{2r^3}\left(1-\frac{h'^2}{r^2}\right).$$

The motion in these magnetic streams is an instance of steady motion for which $udx+vdy+wdz$ may be assumed to be an exact differential. This may be maintained on the principle that, after the impulse is given to the fluid within the magnet in the direction of its axis, the consequent curved courses of the lines of motion are determined solely by the mutual action of the parts of the fluid. Also there may be reason to conclude that for fluid of unlimited extent that expression is an exact differential in any case in which the lines of motion may be cut by surfaces of continuous curvature—that is, whenever the motion is proper to a fluid, and not such as a fluid is capable of when it may be conceived to consist of parts that are solid. Leaving, however, this point for future consideration, I shall now assume, for the reason given above, that $udx+vdy+wdz$ is an exact differential for magnetic streams. In that case, as is known, the relation between the density ρ_1 and velocity V_1 for the streams of a given magnet is expressed by the equation

$$\rho_1=\rho_0 e^{-\frac{V_1^2}{2a^2}},$$

ρ_0 being the density where the fluid is undisturbed. So for another set of streams

$$\rho_2 = \rho_0 e^{-\frac{V_2^2}{2a^2}}.$$

But the steady motions to which these formulæ apply may *coexist*. (This proposition I have proved in the Philosophical Magazine for February 1861, and in the 'Principles of Mathematics,' p. 242.) Consequently the differential

$$(u_1+u_2)\,dx + (v_1+v_2)\,dy + (w_1+w_2)\,dz$$

applies to the steady motion compounded of the two sets, and is plainly an exact differential. Hence if ρ' be the resulting density and V' the resulting velocity, we have

$$\rho' = \rho_0 e^{-\frac{V'^2}{2a^2}}.$$

Having determined the character of the magnetic streams of a cylindrical magnet, and the laws of the composition of such streams, we are prepared to investigate the mechanical action of one cylindrical magnet on another. I shall confine myself to the two instances of the disturbance of a moveable magnet by a fixed one, relative to which Gauss has obtained numerical determinations. (See Gauss's 'Absolute Measure of the Intensity of Terrestrial Magnetism,' Göttingen, 1833; and the *Annales de Chimie et de Physique*, vol. lvii. pp. 56 & 57.) In these experiments the magnets were about a foot long, and the different distances between their middle points varied from four feet to thirteen feet. In both sets the moveable needle when undisturbed was in the plane of the magnetic meridian, the end I have called A being northward, and the end B southward. Also both needles were horizontal with their axes in the same plane.

In the *first* set of experiments the axis of the fixed needle was perpendicular to the plane of the magnetic meridian, and pointed to the middle of the moveable needle. Let us take the case of the experiments made when the fixed needle was on the east side of the moveable one, and its end B (from which the current flows) was turned towards the latter. There were three other cases of relative positions of the magnets; but this one will suffice for my purpose. We have next to determine the action of the composite streams on the individual atoms of the moveable needle, so far as such action tends to move the needle as a whole about a vertical axis. The diameter of each needle is supposed to be small compared with its length.

At the position of any atom of the moveable needle let the velocity of the fluid due to the fixed needle be resolved into u_1 parallel to the axis of the former, v_1 perpendicular to this axis, and w_1 in the vertical direction; and let u_2, v_2, w_2 be the analo-

gous resolved velocities due to the moveable needle. Then, ρ' and V′ being the density and velocity at that position, by what is shown above

$$\rho' = \rho_0 e^{-\frac{V'^2}{2a^2}} = \rho_0\left(1 - \frac{V'^2}{2a^2}\right) \text{ nearly,}$$

and

$$1 - \frac{\rho'}{\rho_0} = \frac{1}{2a^2}\{(u_1+u_2)^2 + (v_1+v_2)^2 + (w_1+w_2)^2\}.$$

Now the velocity and density being functions of space only, it is easy to see that the accelerative action on any atom must have a constant ratio to the acceleration of the fluid where the atom is situated. I have found that this ratio is independent of the magnitude of the atom (Principles of Mathematics, p. 315). As the moveable needle is capable of motion only about a vertical axis through its middle point, we are concerned exclusively with a force proportional to $-\frac{a^2 d\rho'}{\rho_0 dy}$, y being the distance from the axis. The stream of the fixed needle is symmetrical with respect to a vertical plane through its axis, and flows nearly perpendicularly to the axis of the moveable needle, so that u_1 is very small at the positions of all its atoms. A little consideration of the courses of the streams will suffice for perceiving that neither the forces proportional to $(u_1+u_2)\left(\frac{du_1}{dy} + \frac{du_2}{dy}\right)$, nor those proportional to $(w_1+w_2)\left(\frac{dw_1}{dy} + \frac{dw_2}{dy}\right)$, produce any momentum of rotation of the needle. Consequently the motion of rotation wholly depends on the forces proportional to

$$(v_1+v_2)\left(\frac{dv_1}{dy} + \frac{dv_2}{dy}\right).$$

Now the forces $v_1\frac{dv_1}{dy}$ evidently produce equal and opposite momenta on the north and south arms of the needle; the same is the case with the forces $v_2\frac{dv_2}{dy}$, because the values of v_2 are equal with opposite signs at equal distances on the opposite sides of the centre of motion. Also the forces $v_2\frac{dv_1}{dy}$ are mutually destructive, because v_2 at any distance from the centre of motion has equal positive and negative values on the opposite sides of the axis. There remains, therefore, only the momentum due to the forces $v_1\frac{dv_2}{dy}$. These will clearly tend to produce rotation,

because, while v_1 retains the same sign, v_2 has equal values with opposite signs for the two arms.

According to the before-supposed positions of the magnets, the stream from the fixed one will oppose the transverse part of the stream from the moveable one on the east side of the north arm, and conspire with it on the west side, so that the pressure, being greater as the composite velocity is less, will be in excess on the *east* side. For like reasons the pressures on the atoms of the south arm will be in excess on their *west* sides. Hence the movement of the needle will be the same as if the pole B of the fixed needle repelled the pole B of the moveable needle and attracted its pole A.

By this reasoning it is shown that the momentum of rotation of the moveable magnet is proportional to the velocity v_1; and from the foregoing mathematical theory it appears that v_1 is inversely proportional to D^3, D being the distance between the centres of the magnets, or, presumably, that

$$v_1 = \frac{\mu}{D^3}\left(1 - \frac{h^2}{D^2}\right).$$

In the *second* set of experiments the fixed needle was placed either to the north or to the south of the moveable one so that the latter pointed to its centre, and the direction of its axis was still perpendicular to the plane of the meridian. In these positions the stream of the fixed needle will cut at right angles the axis of the moveable one, and its action on the latter will be very nearly the same in kind as in the former set of experiments, but will differ in the circumstance that the velocity at the distance D is *half* the velocity in the other case at the same distance. The more exact proportion of the momenta of rotation in the two cases for the same value of D is presumed to be

$$2 \cdot \frac{1 - \frac{h^2}{D^2}}{1 - \frac{h'^2}{D^2}}, \text{ or } 2\left(1 - \frac{(h^2 - h'^2)}{D^2}\right) \text{ nearly.}$$

These results agree with Gauss's numerical determinations both as regards *the law of the inverse cube* and the *ratio* of the momenta of rotation. This ratio is shown by the experiments to be nearly equal to 2, and to be less than this value by a greater quantity as the distance D is less; which accords with the above expression, if h^2 be greater than h'^2.

The hydrodynamical theory of magnetism has thus given intelligible reasons for the facts of these experiments. The provisional assumption that $VR^3 =$ a constant, for the approximate truth of which an antecedent reason was assigned, seems by these results

to be proved to be the expression of an actual law. In Gauss's theory analogous results are obtained on the hypothesis of two magnetic fluids, which are assumed to be capable of separation, and to be such that, when separated, like fluids mutually repel, and unlike mutually attract, according to the law of the inverse square. But what are we to understand by the separation of dissimilar fluids, and the dependence of mutual attractions and repulsions on this condition? It is as hard to conceive of reasons for these hypotheses as to account for the magnetic facts proposed to be explained by them. The present theory tends to show that there is no physical foundation for such hypotheses, the facts admitting of explanation on the supposition that a single fluid (the æther) acts in a manner conformable to hydrodynamical principles. The argument contained in this communication I am entitled, I think, to regard as confirmatory of the hydrodynamical theory of magnetism.

Cambridge, May 22, 1869.

VI. *Note on the Experimental Illustration of the Expansion of Palladium attending the Formation of its Alloy with Hydrogenium.* By W. Chandler Roberts, *F.C.S., F.G.S.**

ATTENTION has recently been directed to the experimental demonstration of the absorption of hydrogen by palladium†. As the present writer has had the privilege of being connected with Mr. Graham's recent researches, he ventures to offer a description of the special arrangements that, from some experience, appear to him best suited to the purpose of illustration.

It will be remembered that Mr. Graham finds palladium, by the occlusion of 936 volumes of hydrogen, to sustain an increase in its linear dimensions of 1·605 on the 100; or assuming the expansion to be equal in all directions, the cubic expansion will be 4·908 on the 100, equal to sixteen times the dilatation of palladium when heated from 0° C. to 100° C. A simple illustration, well adapted for lecture-experiments, consists in arranging two fine palladium wires on the same plane, but slightly inclined towards each other; these are placed in a cell filled with acidulated water, which may be illuminated by an electric or other lamp, and the image of the wires thrown upon a screen. The wires are to be connected with either element of a small battery, a commutator intervening.

* Communicated by the Author.

† James Dewar, F.R.S.E., "On the Motion of a Palladium Plate during the Formation of Graham's Hydrogenium;" and Poggendorff, "On the Voltaic Deportment of Palladium;" Phil. Mag. No. 251, pp. 424 and 474.

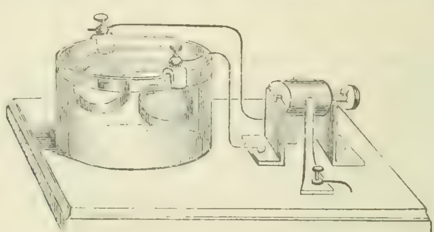
On completion of the circuit the following facts will be observed: from the positive wire, gas (oxygen) is freely evolved, while the negative wire is perfectly quiescent, the hydrogen being for some time entirely absorbed by the metal. When the hydrogen makes its appearance it rises from the end nearest to the positive electrode.

On reversing the direction of the current, evolution of gas ceases from both wires, the hydrogen being occluded by the one, and the oxygen being consumed by the previously absorbed hydrogen in the other*. Attention should also be directed to the flexure produced by the unequal absorption of gas on different sides of the wire.

To obtain a direct demonstration of the expansion, the writer availed himself of the deportment of a compound riband of palladium and platinum when made to form the negative electrode of a battery decomposing acidulated water. The riband consists of two strips, one of palladium, the other of platinum-foil, 300 millims. long, 3 millims. wide; these are soldered together and coiled into a circle, the palladium being inside. If, in the first instance, the coil be connected with the zinc end of the battery, hydrogen will be thrown on the surface of the palladium, which absorbs the gas, and, by the consequent expansion of that metal only, opens the coil, the motion being rendered visible by a light moving index.

On reversing the direction of the current, oxygen will be thrown on the compound riband, and by its combination with the previously absorbed hydrogen, will relax the spiral and cause the index to move rapidly back to zero.

But the employment of an index to magnify the motion is scarcely necessary with so rapid an angular velocity at command. The simplest form, and at the same time the most efficient, consists in placing as the electrodes two strips of palladium-foil varnished on one side and coiled into spirals (each 300 millims. by 5 to 7 millims.) as indicated in the figure†. When one of the strips is



* This experiment was shown at the Meeting of the British Association at Norwich, August 1868.

† As the varnish soon becomes cracked and detached from the foil, it is

uncoiling, the other rolls up on itself. These effects are comparatively slow at first; but as the molecular state of the strips is gradually altered, the evolutions are performed through a large sweep with singular rapidity.

The most striking experiment of all is afforded by the fact that an electrodeposited film of extreme tenuity is capable of occluding hydrogen, and at the same time possesses sufficient tenacity to produce by its expansion a very considerable amount of motion.

A thin strip of platinum-foil, 200 millims. long by 4 millims. wide, was coiled into a circle (like a watch-spring), the external periphery being varnished. Upon the exposed surface a thin film of palladium was deposited by a small battery ($\frac{1}{2}$ litre Bunsen) from a solution of about 1·6 per cent. of the chloride of palladium, the time of exposure being six minutes. The positive pole was represented by a fine platinum wire, a very small portion of which was immersed. A grey coherent film was thus obtained. The strip was then placed in acidulated water and connected with the zinc end of a small battery.

In consequence of its absorption, there was no evolution of gas from its surface; but the metal instantly uncoiled itself, the unattached end passing through an arc of 65°.

On reversing the direction of the current, the strip as rapidly returned to its normal position. The tenacity of the film soon becomes impaired.

In order to give an estimate of the thickness of the film, a sheet of platinum-foil, 20 millims. × 20 millims., having therefore on both sides a surface of 800 square millims., was accurately weighed on a delicate assay-balance at the Mint and exposed for six minutes, as in the case of the strip, to the chloride-of-palladium solution. The foil, after washing in distilled water and drying *in vacuo*, showed an increase in weight of 0·0009 grm.

The following calculation gives the thickness of the film capable of producing so remarkable a result.

Weight of the palladium / Sp. gr. assumed to be . . .

$$\frac{\overset{\text{grm.}}{0{\cdot}0009}}{11{\cdot}8} = 0{\cdot}0000762 \text{ cub. centim.,}$$

or ·0762 cubic millimetre.

Surface . . . . .

$$\frac{{\cdot}0762}{800} = 0{\cdot}000095 \text{ of a millim. thick,}$$

or $\frac{1}{10526}$ of a millimetre.

For the sake of comparison

gold leaf $= \frac{1}{273224}$ inch $= \frac{1}{10656}$ millimetre.

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better (before varnishing) to cover one side of the palladium strip with a thin layer of solder, although the simplicity of the arrangement is to some extent sacrificed.

VII. *On the Polarization of Light by Air mixed with Aqueous Vapour.* *By* Professor HAIDINGER*.

To Professor Tyndall, F.R.S.

Dormbach near Vienna,
June 13, 1869.

MY DEAR SIR,

YOUR late experiments and reports on the polarization of light by cloudy matter (Proceedings of the Royal Society, No. 108, vol. xiii. pp. 223 &c., Jan. 14, 1869) have made a deep impression on my mind.

Permit me to advert to an ancient observation of mine relating to a subject of the kind, but under circumstances widely different, which nevertheless I now very much should wish you may think worthy of a glance in the development of your further inquiries.

I have observed the polarization of light by air mixed with watery vapour. I gave an account of it in Poggendorff's *Annalen* for 1846, vol. lxviii. pp. 73–87 (77). Abbé Moigno, likewise, from Poggendorff, gave a report of it in the fourth volume of his *Répertoire d'Optique Moderne,* 1850, pp. 1338 & 1339. Both were accompanied with diagrams. In the vapour-bath, of course, I had no optical apparatus with me; but having shortly before been struck with the appearance of the brushes of polarized light, or of polarization (*Polarisations-büschel*), I was well prepared to test or to recognize polarized light under certain circumstances with the naked eye, by trying whether I could not see these brushes.

It is perhaps hardly discreet of me to demand you should be at the trouble of searching out old volumes; so I beg you will permit me just to translate that portion of one of my old papers which refers to the subject.

"Brushes of polarization observed in watery vapour.

"White bows or nebulous arches (*Nebelbogen*) have been observed in fogs or mists, having nearly the apparent diameter of rainbows. The light of the rainbow has been found to be polarized by Biot and Sir David Brewster, conformably to the well-known explanation by single reflection of the light of the sun for the interior rainbow, and by double reflection for the exterior rainbow.

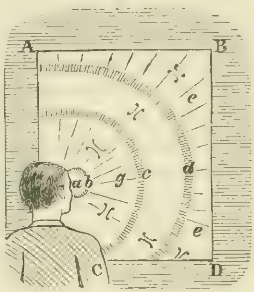
"I had an opportunity to observe the white vapour-bows or arches in the vapour-baths of the 'Sorbienbad,' a most meritorious establishment, conducted by M. Marawetz in the suburb Landstrasse in Vienna. Since my observation a new building has been raised on the east side, so that it is no longer possible there to repeat the observation.

"The sun shone bright at 7 o'clock in the morning, under a

* Communicated by Professor Tyndall.

small elevation through the window into the vapour. A beautiful circular arch presented itself to the eye, the centre of which was the shadow of the head. I endeavoured to represent it in the diagram fig. 1, A B C D being the projection of the window upon the wall on the opposite side of the room.

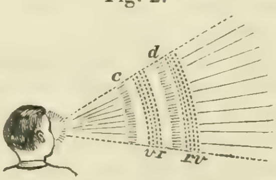
Fig. 1.



"The colour of the arch *f* is a pale bluish white. It is slightly fringed on both sides with a pale orange or brownish yellow, not over bright. The space *e* without and the space *g* within the arch is inferior in light, and of a grey, rather reddish colour. Opposite to the eye, the sun just grazing the eye, there appears a brighter circular spot *a*, fringed at *b* with the slight yellowish or reddish tint. Beginning from *b*, the light is distinctly polarized. The brushes of polarization are quite visible if the eye from one place or direction is quickly directed to another. The brushes have a direction corresponding to the radius in the whitish arch, and a tangential direction in the spaces within and without it. The light of the arch appears, then, to be polarized by reflection from the surface of the particles of vapour or water. The spaces without and within the arch appear, therefore, to be polarized by transmission perpendicularly to the polarization of the arch. The bluish-white and the reddish tints may be faint mixtures of the bluish or reddish fringes of diffraction, combined with the direct reflection from the watery particles floating in the air.

"It is well known that a real rainbow may be produced on a small scale by taking some water in the mouth and then forcibly spouting or puffing it out reduced to the finest watery dust or powder. I availed myself of this method to ascertain, at least approximately, the diameter of the nebulous arch, being without any other apparatus in a vapour-bath. The nebulous arch still continued visible, as in fig. 2; but the first or interior rainbow now became visible, and was situated pretty much in the central line of the nebulous arch; the exterior rainbow, visible only in faint traces, appeared beyond the nebulous arch. The angular values of the semidiameters being for the red of the interior rainbow 42° 2′, for its breadth 1°45′, for the red of the outer rainbow 50° 58′, and its breadth 3° 10′, for the distance of the two rainbows 8° 15′, the breadth of the nebulous arch is consequently

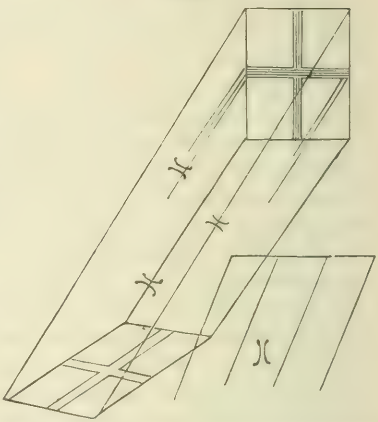
Fig. 2.



equal to about 12°, its central line being nearly at the angular distance of 41° from the centre. But I must claim for these angular values only the character of approximations, as I could only note the data from memory, and did not succeed in getting another sight of the phenomenon.

"In the situation fig. 3, looking at the column of air loaded with vapour and obliquely illuminated by the sun entering through a small window, the transverse brushes of polarization produced by transmission were distinctly visible at *a*, while from the wet boards of the floor at *b* the polarization of reflection was as distinctly visible in the longitudinal brushes.

Fig. 3.



"In a manner somewhat analogous to the preceding observations, the tangential or transverse brushes of polarization may be observed near the sun in vapoury air, while the sun itself is screened from the eye of the observer by intervening objects."

You see, my dear Sir, I have reported only the bare observation, and that only for the sake of following up the "brushes of polarization." But I have not found myself either sufficiently prepared nor prompted by circumstances to follow up the study of the subject itself in the manner it well deserves. You are now in the course of the most interesting inquiries; and I should be happy to find that you would give some kind glance at my own long ago brought forward and now nearly antiquated endeavours.

I still retain the most lively recollection of your friendly visit at my house in Vienna in 1856, when I still was laid up in my bed from the cold I had caught the first day of the opening of our scientific association. And greatly interested I was at so many of your investigations, several of which I had the good luck to quote in confirmation of my humble contributions. Perhaps I should have written this letter in German, so completely are you master of my own language, but I thought this mode of writing would be more in agreement with your daily general practice and intercourse.

Believe me ever, my dear Sir,

Yours very truly,

W. Haidinger.

VIII. *On Ammonium Alloys, and on Nascent-Hydrogen Tests.* *By* Albert H. Gallatin, *M.D., of New York**.

BERZELIUS and De Pontin in 1808, using the voltaic current as Davy had done, endeavoured to do as much for the ammoniacal compounds as he had done for those of the fixed alkalies. They made what is known as the ammoniacal amalgam. That ammonium exists in this body has never been demonstrated, notwithstanding that its constituents in their proper proportions were always found escaping from the amalgam: that does not prove that they were united; on the contrary, 2 vols. of NH^3 and 1 vol. of H are the products. Moreover, if it were ammonium, it had never been made to unite with any other metal than mercury. I have endeavoured to overcome both of these objections.

1. *On the Existence of Ammonium in the Ammoniacal Amalgam, and on a new Test for the presence of Nascent Hydrogen.*

If the hydrogen escaping from the mercury together with the ammonia can be shown to be in the nascent state, it would be evidence that it had just been in chemical combination with the ammonia, in other words, that metallic ammonium (NH^4) existed in the amalgam. Some pellets of sodium were placed in contact with some particles of the transparent variety of phosphorus, wrapped in bibulous paper and plunged beneath the surface of water. A red glow was seen; and as the nascent hydrogen from the decomposing water came into contact with the phosphorus, bubbles of phosphide of hydrogen were formed. Occasionally one would inflame as it came into contact with the atmosphere, placing the nature of the reaction beyond doubt. As phosphide of hydrogen cannot be formed by direct synthesis if ordinary free hydrogen be employed, this becomes a test for the presence of that gas in its nascent state. The hydrogen escaping from the ammoniacal amalgam was now tested by this process. A sodium-amalgam dipped beneath a solution of chloride of ammonium was employed; and it became necessary to wait until the sodium was exhausted, that results might not be vitiated by the nascent hydrogen escaping from the water. At the proper time the decomposing amalgam was covered with fragments of transparent phosphorus, when many bubbles of inflammable phosphide were obtained. The hydrogen must then have been in the nascent state and just escaping from the ammonium.

* Communicated by the Author.

2. *On the Existence of an Alloy of Ammonium and Bismuth, and on another new Test for the presence of Nascent Hydrogen.*

Ammonium had never yet been seen united with any other metal than mercury. Mercury being the only metal fluid at ordinary temperatures, should another alloy be formed it would be a solid. Some bismuth was melted in a porcelain dish and alloyed with sodium by dropping a piece of that metal on the clear surface of the fluid bismuth. Chloride of ammonium was then dusted on the fluid alloy, and then water added in a fine quick stream. The bismuth swells, appears pasty and porous, and then congeals. Abundance of hydrogen escapes from the water, and the ammoniacal odour is set free. This body must now be dried. If it be placed near the ear a distinct crackling noise will be heard, a phenomenon which endures for some days. To ascertain if this be ammonium escaping from the bismuth, the body was placed beneath the surface of water, when bubbles of hydrogen escaped, easily to be collected and recognized; the ammonia, if any, must have been absorbed by the water. To test for this red litmus-paper was placed in the liquid. Wherever the currents from the bismuth struck it a blue spot became visible. On dissolving sulphate of copper in distilled water and placing the well-dried bismuth therein, the characteristic flocculi of ammonio-sulphate of copper appeared at once.

It remains to show that the hydrogen escaping is in the nascent state. There was not enough of it to test with phosphorus. The bismuth compound, when placed in a solution of sulphate of copper, becomes rapidly coated with metallic copper. Now bismuth unalloyed will not precipitate copper from its sulphate. To test if the precipitation of the metallic copper was due to the presence of nascent hydrogen, an alloy of bismuth and sodium was made and dipped in a solution of sulphate of copper. It instantly became coated with that metal, owing to the nascent hydrogen escaping from the water. The hydrogen was therefore escaping in the nascent state from the bismuth and ammonia, and therefore it was a true alloy of bismuth and ammonium. If the temperature of this alloy be raised, it will rapidly decompose with a crackling noise. On one occasion it exploded, sharply scattering the metal. The loud crackling noise produced by this substance may be heard for many days after it is made. That there is no mere surface-action in the case of the mercurial and bismuth alloys of ammonium, is shown by the pores which are formed by the escaping gases in both cases. In the amalgam these pores may be seen produced by the escaping ammonium long after the water has exhausted the sodium. In the mercurial body the pores are evanescent; in the case of bismuth they

remain, and may be examined at leisure. These are different phenomena from those displayed by spongy platinum when it forces hydrogen and oxygen to combine.

Appendix.—Continuation of the investigation at the laboratory of the Royal Mint, London, by the kind permission of Mr. Roberts :—

The alloy was dried *in vacuo* over sulphuric acid. It was then heated *in vacuo* by means of a Sprengel pump, when it decomposed, and the resulting gas was collected over mercury. It was found to have twenty-seven times the volume of the original solid. Analysis of the gas proved it to contain nitrogen and hydrogen. The results of a further examination will shortly be given.

June 23, 1869.

IX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxxvii. p. 474.]

Jan. 28, 1869.—John Peter Gassiot, Esq., Vice-President, in the Chair.

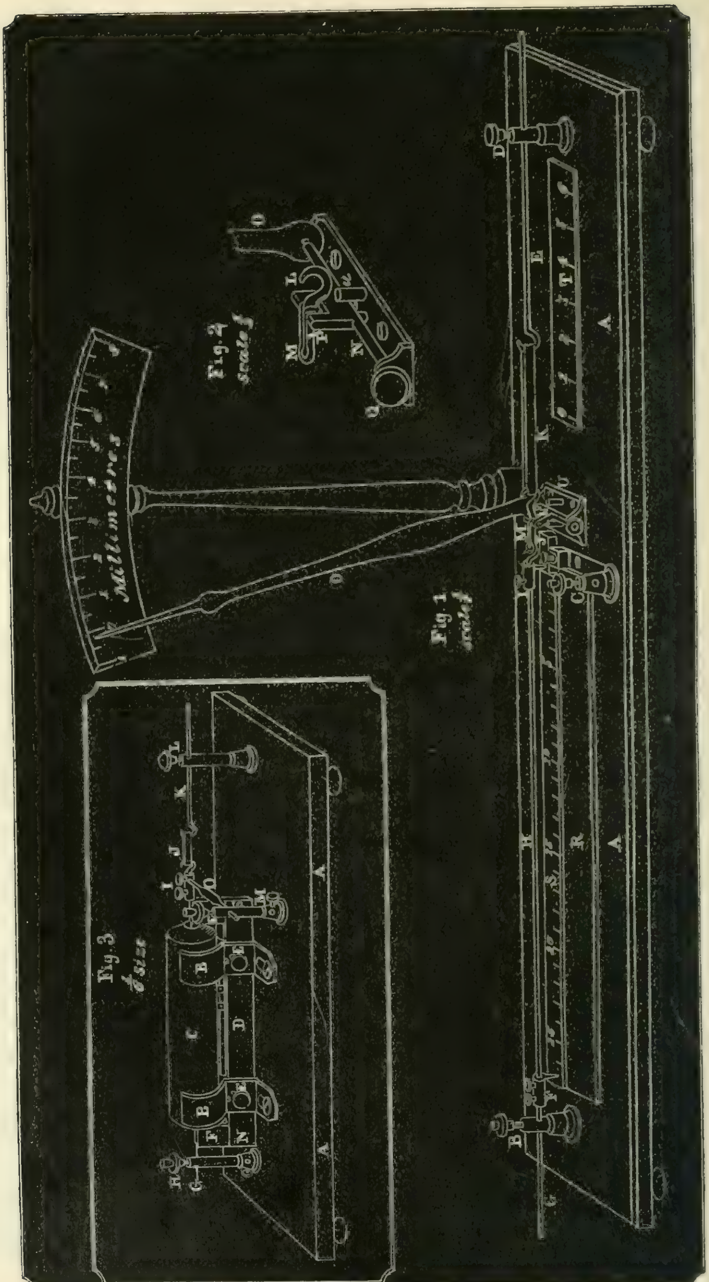
THE following communications were read :—

"On a momentary Molecular Change in Iron Wire." By G. Gore, F.R.S.

Whilst making some experiments of heating a strained iron wire to redness by means of a current of voltaic electricity, I observed that, on disconnecting the battery and allowing the wire to cool, during the process of cooling the wire *suddenly elongated*, and then gradually shortened until it became quite cold.

On attempting, some little time afterwards, to repeat this experiment, although a careful record of the conditions of the experiment had been kept, it was with some difficulty, and after numerous trials, that I succeeded in obtaining the same result. Having again obtained it, I next examined and determined the successful conditions of the experiment, and devised the following arrangement of apparatus.

A A (fig. 1) is a wooden base 61 centimetres long and 15·5 centimetres wide. B and C are binding-screws ; they are provided with small brass mercury-cups fixed in the heads of the screws for attachment of the wires of a voltaic battery. D is a binding-screw for holding fast the sliding wire hook E. F is a cylindrical binding-screw, fixed to the sliding wire G, which is held fast by the binding-screw B. H is the iron or other wire (or ribbon) to be heated : one end of this wire passes through the screw F and is tightly secured by it, whilst the other end is held fast by the cylindrical binding-screw I; the binding-screw I has a small projecting bent piece of copper wire


Millimetres
Fig. 1
Fig. 2
Fig. 3

secured to it, which dips into a little shallow dish or cup of mercury, J; and the mercury in this cup is connected by a screw and strip of brass to the binding-screw C. K is a stretched band of vulcanized india-rubber, attached at one end to the hook of the wire E, and at the other end to the hook L (see fig. 2). The cylindrical binding-screw I has a hook by which it is attached to the loop M (fig. 2). N is an axis suspended delicately upon centres, and carrying a very light index pointer O. The hook L and loop M are separate pieces of metal, and move freely upon an axis, P (fig. 2). The distance from the centre of the axis N to that of P is 12·72 millimetres (=0·5 inch), and to the top of the index pointer 25·45 centimetres (=10·0 inches); every movement horizontally, therefore, of the loop M is attended by a movement, twenty times the amount, of the top of the pointer. Q is a screw for supporting the axis N. I have found it convenient to put the zero-figure of the index towards the left-hand side of the index-plate. R is a separate piece of wood fitting into a rectangular hole in the base-board; it carries a graduated rule, S, for measuring the length of the wire to be heated, and is easily removed, so that the wire may, if necessary, be heated by means of a row of Bunsen's burners. The rule T is used when measuring the amount of strain. U is a vertical stud or pin of brass (of which there are two) for limiting the range of movement of the pointer O.

In using this apparatus, a straight wire or ribbon, H, of a suitable length and thickness was inserted, the index pointer brought to 0 by adjustment of the sliding wire G, and a suitable amount of strain (varying from less than two ounces to upwards of twenty) put upon the wire by adjusting the sliding hooked wire E. One pole of a voltaic battery, generally consisting of six Grove's elements, was connected with the binding-screw C, and the other pole then inserted in the mercury-cup of B. As soon as the needle O attained a maximum or stationary amount of deflection, the battery-wire was suddenly removed from B, and the wire allowed to cool. The movement of the needle O was carefully watched both during its movement to the right hand and also during its return, to see if any irregularity of motion occurred.

Wires of the following metals and alloys were employed:—palladium, platinum, gold, silver, copper, iron, lead, tin, cadmium, zinc, brass, german-silver, aluminium, and magnesium; metallic ribbon was also employed in certain cases.

In these experiments the thickness and length of the conducting-wire or ribbon had to be carefully proportioned to the quantity and electromotive power of the current, so as to produce in the first experiments with each metal only a very moderate amount of heat; thinner (and sometimes also shorter) wires were then successively used, so as ultimately to develope sufficient heat to make the metal closely approach its softening or fusion-point. The battery employed consisted in each case of six Grove's cells, each cell containing two zinc plates $3\frac{3}{4}$ inches wide, and a platinum plate 3 inches wide, each immersed about 5 inches in their respective liquids. The amount of tension imparted by the elastic band required to be carefully ad-

justed to the cohesive power of each metal; if the stretching power was too weak, the phenomenon sought for was not clearly developed; and if too great, the wire was overstretched or broken when it approached the softening-point. The amount of strain imparted was approximately measured by temporarily substituting the body of a small spring balance for the hooked wire F. The heated wire must be protected from currents of cold air.

With wires of iron 0·65 millimetre thick (size "No. 23") and 21·5 centimetres long, strained to the extent of 10 ounces or more, and heated to full redness, the phenomenon was clearly developed. As an example, the needle of the instrument went with regularity to 18·5 of index-plate; the current was then stopped; the needle instantly retreated to 17·75, then as quickly advanced to 19·75, and then went slowly and regularly back, but not to zero. If the temperature of the wire was not sufficiently high, or the strain upon the wire not enough, the needle went directly back without exhibiting the momentary forward movement. The temperature and strain required to be sufficient to actually stretch the wire somewhat at the higher temperature. A higher temperature with a less degree of strain, or a greater degree of strain with a somewhat lower temperature, did not develope the phenomenon; the wire was found to be permanently elongated on cooling. The amount of elongation of the wire during the momentary molecular change was usually about $\frac{1}{240}$ part of the length of the heated part of the wire; but it varied in different experiments; it was greatest in amount when the maximum degrees of strain were applied. The molecular change evidently includes a diminution of cohesion at a particular temperature during the process of cooling; and it is interesting to notice that at the same temperature during the *heating*-process no such loss of cohesion (nor any increase of cohesion) takes place; a certain temperature and strain are therefore not alone sufficient to produce it; the condition of *cooling* must also be included. The phenomena which occur during cooling are not the exact converse of those which take place during heating.

The phenomenon of elongation of iron wire during the process of cooling evidently lies within very narrow limits; it could only be obtained (with the particular battery employed) with wires about 21·5 centimetres ($=8\frac{7}{16}$ inch) long, and about 0·65 millimetre (=Nos. 22 & 23 of ordinary wire-gauge) thick, having a strain upon them of 10 ounces or upwards; with a weaker battery the phenomenon could only be obtained by employing a shorter and thinner wire.

The experiment may easily be verified in a simpler manner by stretching an iron wire about 1·0 millimetre diameter between two fixed supports, keeping it in a sufficient and proper degree of tension by means of an elastic band, then heating it to full redness by means of a row of Bunsen's burners, and, as soon as it has stretched somewhat, suddenly cutting off the source of heat. In some experiments of this kind, with a row (42 centimetres long) of 21 burners and a row (76 centimetres long) of 43 burners, and the wire attached

to a needle with index-plate, as in the figure, conspicuous effects were obtained; but the momentary elongation was relatively much less (in one instance $\frac{1}{600}$ of the length of the heated part) than when a battery was employed, apparently in consequence of the wire being less intensely heated.

A large number of experiments were made with wires of palladium, platinum, gold, silver, copper, lead, tin, cadmium, zinc, brass, german-silver, aluminium, and magnesium (wire and ribbon), diminishing the length and thickness of the wire in each case, and adjusting the tension until suitable temperature and strain were obtained; but in no instance could a similar molecular change to that observed in iron be detected. Palladium and platinum wires of different lengths, thickness, and degrees of strain were examined at various temperatures, up to that of a white heat; but no irregularity of cohesion, except that of gradual softening at the higher temperatures, was observed; they instantly contracted with regular action on stopping the current. Several gold wires were similarly examined at different temperatures up to that of a full red heat; no irregularity occurred either during heating or cooling; but little tension (about 4 ounces) was applied, on account of the weak cohesion of this metal. Wires of silver similarly examined would only bear a strain of about 2 ounces, and a temperature of feeble red heat visible in daylight; no irregularity of elongation or contraction occurred during heating and cooling. By employing exactly the proper temperature and strain, a very interesting phenomenon was observed: the wire melted distinctly *on its surface* without fusing in its interior, although the surface was most exposed to the cooling influence of the air; this occurred without the wire breaking, as it would have done if its interior portion had melted: the phenomenon indicates the passage of the electricity by the *surface* of the wire in preference to passing by its interior. Wires of copper expanded regularly until they became red-hot; they then contracted slightly (notwithstanding the strain applied to them), probably in consequence of a cooling effect of increased radiation produced by the oxidized surface, as a similar effect occurred with brass and german-silver*. On stopping the current the wire contracted without manifest irregularity. Wires of lead and tin were difficult to examine by this method, on account of their extremely feeble cohesion and the low temperature at which they softened: wires about 1·63 millimetre diameter, 25·5 centimetres long (with a strain upon them of about one ounce), were employed; no irregularity was detected. Wires of cadmium from 1·255 millimetre to 1·525 millimetre thick, and 24·2 centimetres long (with a strain of two ounces), exhibited a slight irregularity of expansion at the lower temperatures; they elongated, and also cooled, with extreme slowness, more slowly than those of any other metal. Wires of zinc exhibited a slight irregularity of expansion, like those of cadmium; the most suitable ones were about 25 centimetres long and 1·2 millimetre in diameter, with a strain of 10 ounces. Wires of brass and

* This supposition does not agree with the results obtained with iron wire, which also oxidizes freely.

german-silver, when heated to redness, behaved like those of copper in expanding regularly until a maximum was attained, and then contracting slightly to a definite point whilst the battery remained connected; on stopping the current they contracted without irregularity. When examined at lower temperatures, with a greater degree of strain, no irregularity was observed. Various wires of aluminium were examined; the most suitable was one 0·88 millimetre thick, 20·4 centimetres long, with a strain of 12 ounces; no irregularity was observed at any temperature below redness; aluminium expanded and cooled very slowly, but less so than cadmium. Various wires and ribbon of magnesium were also examined below a red heat, but no irregularity of cohesion, except that due to gradual softening by heat, was detected.

All the metals examined exhibited gradual loss of cohesion at the higher temperatures if a suitable strain was applied to develope it. It is probable that if the fractions of time occupied by the needle in passing over each division of the index were noted, and the wire perfectly protected from currents of air, small irregularities of molecular or cohesive change might be detected by this method; cadmium and zinc offer a prospect of this kind.

This molecular change would probably be found to exist in large masses of wrought iron as well as in the small specimens of wire which I have examined, and would come into operation in various cases where those masses are subjected to the conjoint influence of heat and strain, as in various engineering operations, the destruction of buildings by fire, and other cases.

"On the Development of Electric Currents by Magnetism and Heat." By G. Gore, F.R.S.

I have devised the following apparatus for demonstrating a relation of current electricity to magnetism and heat.

A A, fig. 3, is a wooden base, upon which is supported, by four brass clamps (two, B, B, on each side), a coil of wire, C; the coil is 6 inches long, $1\frac{1}{2}$ inch in external diameter, and $\frac{3}{8}$ of an inch internal diameter, lined with a thin glass tube; it consists of 18 layers, or about 3000 turns of insulated copper wire of 0·415 millim. diameter (or size No. 26 of ordinary wire-gauge); D is a permanent bar-magnet held in its place by the screws E, E, and having upon its poles two flat armatures of soft iron, F, F, placed edgewise. Within the axis of the coil is a straight wire of soft iron, G, one end of which is held fast by the pillar-screw H, and the other by the cylindrical binding-screw I; the latter screw has a hook, to which is attached a vulcanized india-rubber band, J, which is stretched and held secure by the hooked brass rod K and the pillar-screw L. The screw H is surmounted by a small mercury-cup for making connexions with one pole of a voltaic battery, the other pole of the battery being secured to the pillar-screw M, which is also surmounted by a small mercury-cup, and is connected with the cylindrical binding-screw I by a copper wire with a middle flattened portion O to impart to it flexibility. The two ends of the fine wire coil are soldered to two small binding-

screws at the back; those screws are but partly shown in the sketch, and are for the purpose of connexion with a suitable galvanometer. The armatures F, F, are grooved on their upper edges, and the iron wire lies in these grooves in contact with them; and to prevent the electric current passing through the magnet, a small piece of paper or other thin non-conductor is inserted between the magnet and one of the armatures. The battery employed consisted of six Grove's elements (arranged in one series), with the immersed portion of platinum plates about 5 inches by 3 inches; it was sufficiently strong to heat an iron wire of 1·03 millim. diameter and 20·5 centims. long to a low red heat.

By making the contacts of the battery in unison with the movements of the galvanometer-needles, a swing of about 12 degrees of the needles each way was obtained. The galvanometer was not a very sensitive one; it contained 192 turns of wire. Similar results were obtained with a coil 8 inches long and $1\frac{1}{4}$ inch in diameter containing 16 layers, or about 3776 turns of wire of 0·415 millim. diameter (or No. 26 of ordinary wire-gauge), and a permanent magnet 10 inches long. Less effects were obtained with a 6-inch coil consisting of 40 layers, or about 10,000 turns of wire 0·10 millim. in diameter, also with several other coils. The maximum effect, of 12 degrees each way, with six Grove's cells in one series was obtained when the wire became visibly red-hot, and this occurred with an iron wire of 1·03 millim. diameter (or No. 19 of ordinary wire-gauge); but when employing ten such cells as a double series of five, the maximum effect was then obtained with an iron wire of 1·28 to 1·58 millim. diameter (size Nos. 17 and 18), the deflection being 16 degrees each way. By employing a still thicker wire and a battery of greater heating-power still greater effects were obtained.

The galvanometer was placed about 8 (and in some instances 12) feet distant from the coil. A reversal of the direction of the battery-current did not reverse or perceptibly affect the current induced in the coil; but by reversing the poles of the magnet, the direction of the induced current was reversed. On disconnecting the battery, and thereby cooling the iron wire, a reversed direction of induced current was produced. By substituting a wire of pure nickel 24·5 centims. long and 2·1 millims. in diameter, induced currents were obtained as with the iron, but they were more feeble. No induced current occurred by heating the iron wire if the magnet was absent; nor was any induced current obtained if the magnet was present and wires of palladium, platinum, gold, silver, copper, brass, or german-silver were heated to redness instead of iron wire, nor with a rod of bismuth of 3·63 millims. diameter enclosed in a glass tube and heated nearly to fusion; it is evident, therefore, that the axial wire must be composed of a *magnetic* metal.

No continuous current (or only a very feeble one) was produced in the coil by *continuously* heating the iron wire. In several experiments, by employing twelve similar Grove's elements as a double series of six intensity, an iron wire of 1·56 millim. diameter was made *bright* red-hot; and by keeping the current continuous until the galvanometer-needles settled nearly at zero, and then suddenly disconnecting

the battery, the needles remained nearly stationary during several seconds, and then went rapidly to about 10: this slow decline of the current during the first few seconds of cooling was probably connected with the "momentary molecular change of iron wire" during cooling which I have described in the preceding paper. The irregularity of movement of the needles did not occur unless the wire was *bright* red-hot, a condition which was also necessary for obtaining the molecular change.

The direction of the current induced by *heating* the iron wire was found by experiment to be the same as that which was produced by removing the magnet *from* the coil; therefore the heat acted simply by *diminishing* the magnetism, and the results were in accordance with, and afford a further confirmation of, the general law, that where-ever there is increasing or decreasing magnetism, there is a tendency to an electric current in a conductor at right angles to it.

February 11.—Dr. W. B. Carpenter, Vice-President, in the Chair.

The following communication was read:—

"Preliminary Note of Researches on Gaseous Spectra in relation to the Physical Constitution of the Sun." By Edward Frankland, F.R.S., and J. Norman Lockyer, F.R.A.S.

1. For some time past we have been engaged in a careful examination of the spectra of several gases and vapours under varying conditions of pressure and temperature, with a view to throw light upon the discoveries recently made bearing upon the physical constitution of the sun.

Although the investigations are by no means yet completed, we consider it desirable to lay at once before the Royal Society several broad conclusions at which we have already arrived.

It will be recollected that one of us in a recent communication to the Royal Society pointed out the following facts:—

i. That there is a continuous envelope round the sun, and that in the spectrum of this envelope (which has been named for accuracy of description the "chromosphere") the hydrogen line in the green corresponding with Fraunhofer's line F takes the form of an arrow-head, and widens from the upper to the lower surface of the chromosphere.

ii. That ordinarily in a prominence the F line is nearly of the same thickness as the C line.

iii. That sometimes in a prominence the F line is exceedingly brilliant, and widens out so as to present a bulbous appearance above the chromosphere.

iv. That the F line in the chromosphere, and also the C line, extend on to the spectrum of the subjacent regions and re-reverse the Fraunhofer lines.

v. That there is a line near D visible in the spectrum of the chromosphere to which there is no corresponding Fraunhofer line.

vi. That are many bright lines visible in the ordinary solar spectrum near the sun's edge.

vii. That a new line sometimes makes its appearance in the chromosphere.

2. It became obviously, then, of primary importance—

i. To study the hydrogen spectrum very carefully under varying conditions, with the view of detecting whether or not there existed a line in the orange, and

ii. To determine the cause to which the thickening of the F line is due.

We have altogether failed to detect any line in the hydrogen spectrum in the place indicated, *i. e.* near the line D; but we have not yet completed all the experiments we had proposed to ourselves.

With regard to the thickening of the F line, we may remark that, in the paper by MM. Plücker and Hittorf, to which reference was made in the communication before alluded to, the phenomena of the expansion of the spectral lines of hydrogen are fully stated, but the cause of the phenomena is left undetermined.

We have convinced ourselves that this widening out is due to pressure, and not appreciably, if at all, to temperature *per se*.

3. Having determined, then, that the phenomena presented by the F line were phenomena depending upon and indicating varying pressures, we were in a position to determine the atmospheric pressure operating in a prominence, in which the red and green lines are nearly of equal width, and in the chromosphere, through which the green line gradually expands as the sun is approached*.

With regard to the higher prominences, we have ample evidence that the gaseous medium of which they are composed exists in a condition of *excessive* tenuity, and that at the lower surface of the chromosphere itself the pressure is very far below the pressure of the earth's atmosphere.

The bulbous appearance of the F line before referred to may be taken to indicate violent convective currents or local generations of heat, the condition of the chromosphere being doubtless one of the most intense action.

4. We will now return for one moment to the hydrogen spectrum. We have already stated that certain proposed experiments have not been carried out. We have postponed them in consequence of a further consideration of the fact that the bright line near D has apparently no representative among the Fraunhofer lines. This fact implies that, assuming the line to be a hydrogen line, the selective absorption of the chromosphere is insufficient to reverse the spectrum.

It is to be remembered that the stratum of incandescent gas which is pierced by the line of sight along the sun's limb, the radiation from which stratum gives us the spectrum of the chromosphere, is very great compared with the radial thickness of the chromosphere itself; it would amount to something under 200,000 miles close to the limb.

Although there is another possible explanation of the non-reversal of the D line, we reserve our remarks on the subject (with which the visibility of the prominences on the sun's disk is connected) until further experiments and observations have been made.

* Will not this enable us ultimately to determine the temperature?

5. We believe that the determination of the above-mentioned facts leads us necessarily to several important modifications of the received theory of the physical constitution of our central luminary—the theory we owe to Kirchhoff, who based it upon his examination of the solar spectrum. According to this hypothesis, the photosphere itself is either solid or liquid, and it is surrounded by an atmosphere composed of gases and the vapours of the substances incandescent in the photosphere.

We find, however, instead of this compound atmosphere, one which gives us nearly, or at all events mainly the spectrum of hydrogen ; (it is not, however, composed necessarily of hydrogen alone ; and this point is engaging our special attention ;) and the tenuity of this incandescent atmosphere is such that it is extremely improbable that any considerable atmosphere, such as the corona has been imagined to indicate, lies outside it,—a view strengthened by the fact that the chromosphere bright lines present no appearance of absorption, and that its physical conditions are not statical.

With regard to the photosphere itself, so far from being either a solid surface or a liquid ocean, that it is cloudy or gaseous or both follows both from our observations and experiments. The separate prior observations of both of us have shown :—

i. That a gaseous condition of the photosphere is quite consistent with its continuous spectrum. The possibility of this condition has also been suggested by Messrs. De La Rue, Stewart, and Loewy.

ii. That the spectrum of the photosphere contains bright lines when the limb is observed, these bright lines indicating probably an outer shell of the photosphere of a gaseous nature.

iii. That a sun-spot is a region of greater absorption.

iv. That occasionally photospheric matter appears to be injected into the chromosphere.

May not these facts indicate that the absorption to which the reversal of the spectrum and the Fraunhofer lines are due takes place in the photosphere itself or extremely near to it, instead of in an extensive outer absorbing atmosphere ? And is not this conclusion strengthened by the consideration that otherwise the newly discovered bright lines in the solar spectrum itself should be themselves reversed on Kirchhoff's theory ? this, however, is not the case. We do not forget that the selective radiation of the chromosphere does not necessarily indicate the whole of its possible selective absorption ; but our experiments lead us to believe that, were any considerable quantity of metallic vapours present, their bright spectra would not be entirely invisible in all strata of the chromosphere.

February 18.—Lieut.-General Sabine, President, in the Chair.

The following communication was read :—

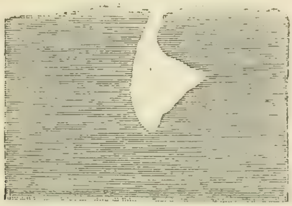
"Note on a Method of viewing the Solar Prominences without an Eclipse." By William Huggins, F.R.S.

Last Saturday, February 13, I succeeded in seeing a solar prominence so as to distinguish its form. A spectroscope was used ; a narrow slit was inserted after the train of prisms before the object-glass of the little telescope. This slit limited the light entering the

telescope to that of the refrangibility of the part of the spectrum immediately about the bright line coincident with C.

The slit of the spectroscope was then widened sufficiently to admit the form of the prominence to be seen. The spectrum then became so impure that the prominence could not be distinguished.

A great part of the light of the refrangibilities removed far from that of C was then absorbed by a piece of deep ruby glass. The prominence was then distinctly perceived, something of this form.



A more detailed account is not now given, as I think I shall be able to modify the method so as to make the outline of these objects more easily visible.

February 25.—Captain Richards, R.N., Vice-President, in the Chair.

The following communications were read:—

"Note on the Heat of the Stars." By William Huggins, F.R.S.

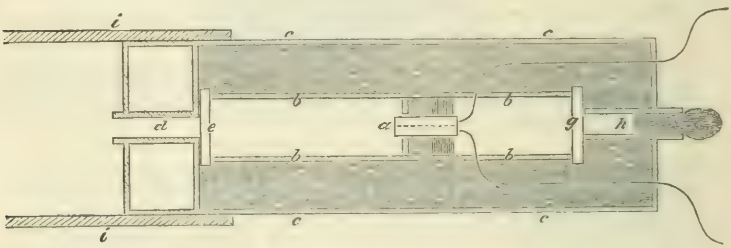
In the summer of 1866 it occurred to me that the heat received on the earth from the stars might possibly be more easily detected than the solar heat reflected from the moon. Mr. Becker (of Messrs. Elliott Brothers) prepared for me several thermopiles, and a very sensitive galvanometer. Towards the close of that year, and during the early part of 1867, I made numerous observations on the moon, and on three or four fixed stars. I succeeded in obtaining trustworthy indications of stellar heat in the case of the stars Sirius, Pollux, and Regulus, though I was not able to make any quantitative estimate of their calorific power.

I had the intention of making these observations more complete, and of extending them to other stars. I have refrained hitherto from making them known; I find, however, that I cannot hope to take up these researches again for some months, and therefore venture to submit the observations in their present incomplete form.

An astatic galvanometer was used, over the upper needle of which a small concave mirror was fixed, by which the image of the flame of a lamp could be thrown upon a scale placed at some distance. Usually, however, I preferred to observe the needle directly by means of a lens so placed that the divisions on the card were magnified, and could be read by the observer when at a little distance from the instrument. The sensitiveness of the instrument was made as great as possible by a very careful adjustment from time to time of the magnetic power of the needles. The extreme delicacy of the instrument was found to be more permanently preserved when the needles were placed at right angles to the magnetic meridian during the time that the instrument was not in use. The great sensitiveness of this in-

strument was shown by the needles turning through 90° when two pieces of wire of different kinds of copper were held between the finger and thumb. For the stars, the images of which in the telescope are points of light, the thermopiles consisted of one or of two pairs of elements; a large pile, containing twenty-four pairs of elements, was also used for the moon. A few of the later observations were made with a pile of which the elements consist of alloys of bismuth and antimony.

The thermopile was attached to a refractor of eight inches aperture. I considered that though some of the heat-rays would not be transmitted by the glass, yet the more uniform temperature of the air within the telescope, and some other circumstances, would make the difficulty of preserving the pile from extraneous influences less formidable than if a reflector were used.



The pile *a* was placed within a tube of cardboard, *b*; this was enclosed in a much larger tube formed of sheets of brown paper pasted over each other, *c*. The space between the two tubes was filled with cotton-wool. At about 5 inches in front of the surface of the pile, a glass plate (*e*) was placed for the purpose of intercepting any heat that might be radiated from the inside of the telescope. This glass plate was protected by a double tube of cardboard, the inner one of which (*d*) was about half an inch in diameter. The back of the pile was protected in a similar way by a glass plate (*g*). The small inner tube (*h*) beyond the plate was kept plugged with cotton-wool; this plug was removed when it was required to warm the back of the pile, which was done by allowing the heat radiated from a candle-flame to pass through the tube to the pile. The apparatus was kept at a distance of about 2 inches from the brass tube by which it was attached to the telescope by three pieces of wood (*i*), for the purpose of cutting off as much as possible any connexion by conduction with the tube of the telescope.

The wires connecting the pile with the galvanometer, which had to be placed at some distance to preserve it from the influence of the ironwork of the telescope, were covered with gutta percha, over which cotton-wool was placed, and the whole wrapped round with strips of brown paper. The binding-screws of the galvanometer were enclosed in a small cylinder of sheet gutta percha, and filled with cotton-wool. These precautions were necessary, as the approach of the hand to one of the binding-screws, or even the impact upon it of the cooler air entering the observatory, was sufficient to

produce a deviation of the needle greater than was to be expected from the stars.

The apparatus was fixed to the telescope so that the surface of the thermopile would be at the focal point of the object-glass. The apparatus was allowed to remain attached to the telescope for hours, or sometimes for days, the wires being in connexion with the galvanometer, until the heat had become uniformly distributed within the apparatus containing the pile, and the needle remained at zero, or was steadily deflected to the extent of a degree or two from zero.

When observations were to be made, the shutter of the dome was opened, and the telescope, by means of the finder, was directed to a part of the sky near the star to be examined where there were no bright stars. In this state of things the needle was watched, and if in four or five minutes no deviation of the needle had taken place, then by means of the finder the telescope was moved the small distance necessary to bring the image of the star exactly upon the face of the pile, which could be ascertained by the position of the star as seen in the finder. The image of the star was kept upon the small pile by means of the clock-motion attached to the telescope. The needle was then watched during five minutes or longer; almost always the needle began to move as soon as the image of the star fell upon it. The telescope was then moved, so as to direct it again to the sky near the star. Generally in one or two minutes the needle began to return towards its original position.

In a similar manner twelve to twenty observations of the same star were made. These observations were repeated on other nights.

The mean of a number of observations of Sirius, which did not differ greatly from each other, gives a deflection of the needle of 2°.

The observations of Pollux $1\frac{1}{2}$°.

No effect was produced on the needle by Castor.

Regulus gave a deflection of 3°.

In one observation Arcturus deflected the needle 3° in 15 minutes.

The observations of the full moon were not accordant. On one night a sensible effect was shown by the needle; but at another time the indications of heat were excessively small, and not sufficiently uniform to be trustworthy.

It should be stated that several times anomalous indications were observed, which were not traced to the disturbing cause.

The results are not strictly comparable, as it is not certain that the sensitiveness of the galvanometer was exactly the same in all the observations, still it was probably not greatly different.

Observations of the heat of the stars, if strictly comparable, might be of value, in connexion with the spectra of their light, to help us to determine the condition of the matter from which the light was emitted in different stars.

I hope at a future time to resume this inquiry with a larger telescope, and to obtain some approximate value of the quantity of heat received at the earth from the brighter stars.

"On the Fracture of Brittle and Viscous Solids by 'Shearing.'" By Sir William Thomson, F.R.S.

On recently visiting Mr. Kirkaldy's testing works, the Grove,

Southwark, I was much struck with the appearances presented by some specimens of iron and steel round bars which had been broken by torsion. Some of them were broken right across, as nearly as may be in a plane perpendicular to the axis of the bar. On examining these I perceived that they had all yielded through a great degree to distortion before having broken. I therefore looked for bars of hardened steel which had been tested similarly, and found many beautiful specimens in Mr. Kirkaldy's museum. These, without exception, showed complicated surfaces of fracture, which were such as to demonstrate, as part of the whole effect in each case, a spiral fissure round the circumference of the cylinder at an angle of about 45° to the length. This is just what is to be expected when we consider that if A B D C (fig. 1) represent an infinitesimal square on the surface of a round bar with its sides A C and B D parallel to the axis of the cylinder, before torsion, and A B D′ C′ the figure into which this square becomes distorted just before rupture, the diagonal A D has become elongated to the length A D′, and the diagonal B C has become contracted to the length B C′, and that therefore there must be maximum tension everywhere, across the spiral of which B C′ is an infinitely short portion. But the specimens are remarkable as showing in softer or more viscous solids a tendency to break parallel to the surfaces of "shearing" A B, C D, rather than in surfaces inclined to these at an angle of 45°. Through the kindness of Mr. Kirkaldy, his specimens of both kinds are now exhibited to the Royal Society. On a smaller scale I have made experiments on round bars of brittle sealing-wax, hardened steel, similar steel tempered to various degrees of softness, brass, copper, lead.

Fig, 1.

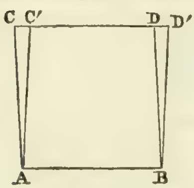
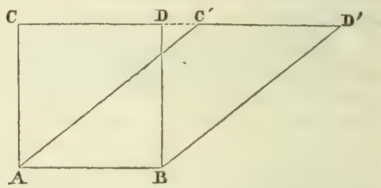


Fig. 2.



Sealing-wax and hard steel bars exhibited the spiral fracture. All the other bars, without exception, broke as Mr. Kirkaldy's soft steel bars, right across, in a plane perpendicular to the axis of the bar. These experiments were conducted by Mr. Walter Deed and Mr. Adam Logan in the Physical Laboratory of the University of Glasgow; and specimens of the bars exhibiting the two kinds of fracture are sent to the Royal Society along with this statement. I also send photographs exhibiting the spiral fracture of a hard steel cylinder, and the "shearing" fracture of a lead cylinder by torsion.

These experiments demonstrate that continued "shearing" parallel to one of planes, of a viscous solid, developes in it a tendency to break more easily parallel to these planes than in other directions, or that a viscous solid, at first isotropic, acquires "cleavage-planes" parallel to the planes of shearing. Thus, if C D and A B

(fig. 2) represent in section two sides of a cube of a viscous solid, and if, by "shearing" parallel to these planes, C D be brought to the position C′ D′, relatively to A B supposed to remain at rest, and if this process be continued until the material breaks, it breaks parallel to A B and C′ D′.

The appearances presented by the specimens in Mr. Kirkaldy's museum attracted my attention by their bearing on an old controversy regarding Forbes's theory of glaciers. Forbes had maintained that the continued shearing motion which his observations had proved in glaciers, must tend to tear them by fissures parallel to the surfaces of "shearing." The correctness of this view for a viscous solid mass, such as snow becoming kneaded into a glacier, or the substance of a formed glacier as it works its way down a valley, or a mass of débris of glacier-ice, reforming as a glacier after disintegration by an obstacle, seems strongly confirmed by the experiments on the softer metals described above. Hopkins had argued against this view, that, according to the theory of elastic solids, as stated above, and represented by the first diagram, the fracture ought to be at an angle of 45° to the surfaces of "shearing." There can be no doubt of the truth of Hopkins's principle *for an isotropic elastic solid, so brittle as to break by shearing before it has become distorted through more than a very small angle*; and it is illustrated in the experiments on brittle sealing-wax and hardened steel which I have described. The various specimens of fractured elastic solids now exhibited to the Society may be looked upon with some interest, if only as illustrating the correctness of each of the two seemingly discrepant propositions of those two distinguished men.

GEOLOGICAL SOCIETY.

[Continued from vol. xxxvii. p. 311.]

Nov. 25th, 1868.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—

1. "On Floods in the Island of Bequia." By G. M. Browne, Esq. Communicated by the Secretary of State for Foreign Affairs.

On the 17th of March, at 8 o'clock P.M., a steady strong wave was seen bearing down upon Admiralty Bay; it had no perceptible crest, and was three feet in height; it encroached upon the land to distances varying from 70 to 350 feet. A second, smaller wave followed. No shock of an earthquake was felt.

2. "Description of Nga Tutura, an Extinct Volcano in New Zealand." By Capt. F. W. Hutton, F.G.S.

This volcano is situated on the west coast of the North Island of New Zealand, between Raglan and the mouth of the River Waikato.

A section of 15 miles is exposed along the coast, which trends in a north-west and south-east direction, showing beds of Mesozoic age forming a synclinal trough between the south head of Waikato and Otehe Point, and descending below the sea-level at Waikawau. Upon them lie Tertiary strata, following the same synclinal

curve as the older rocks, and broken through, nearly in the centre of the curve, by the basaltic cone of Nga Tutura. This volcano is about 600 feet high, and is chiefly composed of basaltic lava-streams, with but little tuff. The eruption is considered by the author to have been submarine.

Capt. Hutton then stated his conviction that the fluid matter which escaped was not connected with a central molten interior of the earth, but was derived from rocks not much more than 1000 feet in depth, and that the synclinal in question was caused by a subsidence into the cavity thus formed.

3. "On *Dakosaurus*." By J. Wood Mason, Esq., F.G.S.

The Kimmeridge Clay of Shotover Hill has yielded five specimens of the teeth of this reptile, now for the first time represented as a British genus. After noticing the bibliography of the subject, and the presence of specimens in various museums, the author proceeded to describe the characters of the teeth. They are large, conical, incurved, and slightly recurved, having two sharp, prominent, crenulated, longitudinal ridges, which are situated midway between the convex and concave curvatures.

This reptile was regarded by the author as foreshadowing the form of dentition that characterizes the existing group of *Varanidæ*. If the materials were at hand for a complete definition of its comparative osteology, *Dakosaurus* would probably exhibit a combination of Lacertilian and Crocodilian characters, but with the crocodilian elements predominant.

The PRESIDENT differed from the author as to the conclusions he drew from the structure of the teeth. The teeth of existing Crocodilia had been but imperfectly described, and he thought he could point out among existing Crocodiles teeth bearing the character which the author regarded as Lacertilian. He agreed with Prof. Owen in regarding *Dakosaurus* as Crocodilian rather than Dinosaurian or Lacertilian.

4. "On the Anatomy of the test of *Amphidetus* (*Echinocardium*) *Virginianus*, Forbes; and on the genus *Breynia*." By P. Martin Duncan, M.B., F.R.S., Sec. G.S., &c.

After a careful examination of the Miocene *Amphidetus* from the Virginian Tertiaries, the recent species of the genus from the European and Australian seas were stated to form a group of very closely allied forms. The Crag specimen of *A. cordatus* described by Forbes could not be found; but the examination of a series of recent specimens decided that they were not specifically different from the Miocene form.

The unusual form of the ambulacral spaces, the nature of the fasciole crossing them, and the resulting absence (more or less) of pores within the fasciole, were asserted to be of a third-rate character as regards structural importance; and the author did not consider that the genera *Echinocardium*, *Breynia*, *Lovenia*, &c. had a common origin or that there was a close genetic relationship between them because they had this fasciolar structure. He con-

sidered the fasciole to be an appendage to several generic groups which were distinctly separated by other structural distinctions. The result of an examination of the Nummulitic *Breyniæ* in the Society's collection satisfied Dr. Duncan that there were only race characters separating them from *Breynia Australiensis*—a recent Echinoderm. The persistence of these species, widely distributed and of great geological age, was very remarkable.

December 9th, 1868.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following communication was read :—

"Notes of a Geological Reconnaissance in Arabia Petræa." By H. Bauerman, Esq., F.G.S.

The district to which this paper referred is that between Suez and the lower part of Wady Ferran in the peninsula of Arabia Petræa, and includes the copper and turquoise mines worked by the ancient Egyptians. The rocks within this area were classified as follows :—

1. Gneiss and granites, forming the central chain of Sinai and the base of all the stratified deposits.
2. Red Sandstone series.
3. Cretaceous rocks.
4. White limestones, with flints, salt, and bitumen. Eocene.
5. Flint conglomerate, with coralline limestone. Miocene.
6. Gypseous marls of Wady Taragi.
7. Reconstructed gypseous sands and conglomerates.
8. Raised beaches, coralline and miliolitic limestones.
9. Alluvium and desert drift.

The Red Sandstone series consists of three members, a thin bed of limestone being the central and containing remains of Encrinites referred by Mr. Etheridge to the Muschelkalk form *Encrinites moniliformis*. Iron, manganese, and copper ores are found near Nasb and Serabib el Khadem. The turquoise mines of Wady Maghara, which were referred to the same horizon, are among the most ancient monuments of the world. The author considered that the tools employed were flint chisels or flakes, and hammers made from pieces of a neighbouring doleritic lava. The flakes were supposed to have been mounted on wooden blocks.

The Cretaceous rocks, which rest unconformably on the Triassic sandstones, consist chiefly of green sand, with alternations of thin argillaceous limestones, containing Echinoderms which prove them to be of the age of the Upper Greensand. Above them comes the Hippurite-limestone series. The fossils were described by Dr. Duncan, F.R.S., in a subsequent communication.

The white limestone, with flints, the next group of rocks in ascending order, strongly resembles the European chalk with flints; but, according to the author, it must be regarded as representing the nummulitic limestone of Egypt, as several species of Nummulites have been detected in it near the shores of the Red Sea, below Wady Gharandel. The Miocene flint conglomerate series is a mass of coarse flint shingle alternating with these coralline limestones. The author considered that a great physical break ensued between

the Eocene and Miocene period, while a gradual transition occurred between the Cretaceous and Eocene rocks.

In the gypseous series which overlies the flint conglomerate several peculiar effects were noted, owing to the easy manner in which tumbled and broken masses of gypsum are reconstructed by partial solution and recrystallization when they have been removed from their original position by the slipping of the underlying shales.

The alluvial gravels of the Sinaitic valleys are generally similar in containing a coarser and a finer material; the latter is the older, and has apparently been deposited by comparatively slow-flowing streams. In conclusion, the author called attention to the evidence of lakes, marshes, and streams having formerly occupied what are now dry barren valleys.

X. *Intelligence and Miscellaneous Articles.*

ON THE HEAT CONSUMED IN INTERNAL WORK WHEN A GAS DILATES UNDER THE PRESSURE OF THE ATMOSPHERE. BY M. J. MOUTIER.

M. CLAUSIUS has shown that the quantity of heat necessary to heat a body consists in general of three distinct parts: the first represents the increase of the quantity of heat actually existing in the interior of the body; the second has for its equivalent the external work, and the third the *internal work*. When a gas dilates under the pressure of the atmosphere, the external work is easily estimated. If we call δ the density of the gas compared with the air, and α the coefficient of dilatation of the gas under the pressure of the atmosphere, the increase of volume experienced by 1 kilogramme of gas in passing from zero to 1° is, in cubic metres, $\frac{\alpha}{1{\cdot}2932\times\delta}$. Moreover the atmospheric pressure upon one square metre is equal to 10333 kilogs.; consequently when 1 kilog. of gas dilates from zero to 1° under the constant pressure of the atmosphere, the external work is equal to $\frac{10333\times\alpha}{1{\cdot}2932\times\delta}$; and the heat consumed in external work is obtained by dividing this number by the mechanical equivalent of heat, 425. If we represent by C the specific heat of the gas under the pressure of the atmosphere, by K the absolute specific heat independent of the physical condition of the body according to M. Clausius, and by γ the heat consumed in internal work, we have, when 1 kilog. of gas dilates by 1° under the pressure of the atmosphere,

$$C=K+\frac{1}{425}\times\frac{10333\times\alpha}{1{\cdot}2932\times\delta}+\gamma. \quad . \quad . \quad . \quad . \qquad (1)$$

This equation contains two unknown quantities, K and γ.

Messrs. William Thomson and Joule have succeeded in demonstrating the existence of internal work in a gas which expands without effecting any external work. The diminution of temperature which accompanies the flow of the gas allowed the calculation of the proportion of the internal to the external work when the gas

dilates with displacement of the point of application of an external pressure; this proportion, which is insensible in the case of hydrogen, is perfectly appreciable with air, and much greater in the case of carbonic acid.

M. Hirn has assumed the internal work to be negligible in hydrogen. He has deduced from the preceding equation the absolute specific heat of that gas; and by applying the law of Dulong and Petit to the absolute specific heats, he has been able to obtain under this hypothesis the values of γ with respect to various gases. By combining the equation (1) with the law of absolute specific heats, we may compare the values of γ for various gases without the assumption of any hypothesis with regard to hydrogen.

Air and Hydrogen.—According to the experiments of M. Regnault, we have, for hydrogen, $C=3{\cdot}409$ between zero and 200°, $\alpha=0{\cdot}003661$ between zero and 180°, and $\delta=0{\cdot}06926$. The equation (1) gives for this gas

$$K=2{\cdot}41523-\gamma. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The experiments of M. Regnault give for air, $C'=0{\cdot}23751$ between zero and 200°, $\alpha'=0{\cdot}00367$ between zero and 100°. The equation (1) applied to this gas gives

$$K'=0{\cdot}168512-\gamma. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Now 100 parts by weight of air contain 77 parts of nitrogen and 23 parts of oxygen; if we apply, with M. Clausius, the law of absolute specific heats to air considered as a compound body, designating by K_1 and K_2 the absolute specific heats of nitrogen and oxygen,

$$100K'=77K_1+23K_2.$$

But if we apply the same law to nitrogen, to oxygen, and to hydrogen, the atomic weights of which are to each other as the numbers 14, 16, and 1,

$$K=14K_1, \quad K=16K_2.$$

By transferring these values of K_1 and K_2 into the preceding equation,

$$K'=0{\cdot}069375\,K;$$

and by replacing K and K' in this last equation by the values deduced from the equations (2) and (3), we have, finally,

$$\gamma'=0{\cdot}069375\gamma+0{\cdot}000956.$$

Carbonic Acid and Hydrogen.—The data furnished by M. Regnault's experiments for carbonic acid are, $C''=0{\cdot}21692$ between 10° and 210°, $\alpha''=0{\cdot}003710$ between zero and 100°, $\delta''=0{\cdot}52901$. The equation (1) gives for this gas

$$K''=0{\cdot}171302-\gamma''. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If we represent by $\frac{1}{2}$ the atomic weight of hydrogen, the mean atomic weight of carbonic acid is $\frac{22}{3}$, and according to the law of absolute specific heats,

$$\tfrac{1}{2}K=\tfrac{22}{3}K''.$$

Replacing K and K'' in this equation by the values deduced from the equations (2) and (4), we have

$$\gamma''=0{\cdot}068181\gamma+0{\cdot}006628.$$

In these calculations the specific heats are taken between zero and 200°, and the coefficients of dilatation are in relation to the interval from zero to 100°; it is probable that between 100° and 200° the coefficients of dilatation of air and hydrogen retain sensibly the same value, and that the coefficient of dilatation of carbonic acid tends to diminish, so that the value calculated for γ'' is a little too small.

Conclusion.—If for each of these three gases (hydrogen, air, and carbonic acid) we take the proportion of the heat consumed in internal work to the specific heat under a constant pressure, we find the following values for $\frac{\gamma}{C'}$, $\frac{\gamma'}{C'}$, $\frac{\gamma''}{C''}$:—

Hydrogen................	0·297
Air	0·297+0·004
Carbonic acid	0·317+0·035

We see, therefore, that the heat consumed in internal work, when the gas dilates under the constant pressure of the atmosphere between zero and 200°, is a fraction of the specific heat under constant pressure, which goes on increasing from hydrogen to air and from air to carbonic acid.

We may likewise compare the quantities of heat expended in internal work under the same circumstances by considering the three gases under the same volume at the temperature of melting ice. If we take as the common volume the volume occupied by 1 kilog. of hydrogen, the weight of equal volumes of air and carbonic acid are respectively

$$\frac{1 \text{ kilog.}}{0\cdot06926} \text{ and } \frac{1 \text{ kilog.}}{0\cdot06926} \times 1\cdot529;$$

and the quantities of heat consumed in internal work are respectively for these three gases, considered under the same volume,

$$\gamma, \ \frac{\gamma'}{0\cdot06926}, \text{ and } \gamma'' \times \frac{1\cdot529}{0\cdot06926},$$

or

Hydrogen.............	γ
Air	$1\cdot0015\gamma+0\cdot013$
Carbonic acid	$1\cdot505\gamma+0\cdot146$.

These quantities of heat likewise increase from hydrogen to air and from air to carbonic acid.

The law of Dulong and Petit applied to absolute specific heats, therefore, leads us to arrange hydrogen, air, and carbonic acid, with regard to internal work, in the order which the experiments of Messrs. W. Thomson and Joule assign to these very gases.——*Comptes Rendus*, January 11, 1869, vol. lxviii. pp. 95–98.

INVESTIGATIONS ON OBSCURE CALORIFIC SPECTRA.

BY M. DESAINS.

I have the honour to lay before the Academy the results of new investigations on obscure calorific spectra. The questions I have deavoured to solve are the following:—

(1) Given, in a spectrum formed by a prism of definite nature and

angle, a group of rays of almost the same refrangibilities, and forming a band of feeble but constant magnitude, to investigate how the calorific action of this band varies with its mean refrangibility on the one hand, and with the nature of the source of heat on the other.

(2) To investigate further how the transmissibility of such rays through a screen of given thickness changes when either their mean refrangibility is varied, or else the nature of the source or that of the absorbent is altered.

The difficulties experienced in these researches are those always met with in attempting to form, with rays other than the solar rays, pure spectra of an intensity sufficient for calorimetric experiments. I do not dare to affirm that I have completely solved these difficulties; but, at any rate, I think I have succeeded in finding the conditions in which the mixture of the rays is so feeble as not to exert an appreciable influence on the result of my experiments.

To produce these spectra I concentrated the rays from the source of heat on a narrow slit. A lens with a focus of about 16 centims. was placed about 30 centims. from the slit, and formed a defined image of it in the conjugate focus. The prism placed behind this lens deflected the rays, and transformed the colourless image into one whose luminous part extended over from 0·015 to 0·025 metre, according to the nature of the prisms used. The thermoscopic pile was linear and very narrow, its aperture being scarcely broader than 0·001 metre.

Under these circumstances the purity of the spectra, and therefore the certainty of the results furnished by analysis, must obviously depend on the breadth of the slit which served as the source of heat. The ideal case would be that in which this slit was infinitely narrow. This cannot be realized; but in all the experiments whose results I am about to indicate, I found that I could vary the breadth of the slit from 0·0005 to 0·0015 metre (that is, in the proportion of 1 : 3) without at all changing the conclusions to which I was led concerning the distribution of heat in the various parts of the spectrum, or regarding the absorptions which the consecutive parts of these layers experience in different media. I think I am thence justified in assuming that in my experiments any injurious influence of the mixture of the rays was eliminated.

I worked with four different sources:—

(1) A thick platinum wire kept at a red heat in the flame of a Bunsen's burner.

(2) A bat's-wing burner with the section turned towards the slit.

(3) An ordinary moderator lamp.

(4) A Bourbouze lamp. The flame of this lamp is a kind of thimble of very close platinum-wire gauze, kept at a red heat by means of a gas-flame fed by compressed air.

With the first two sources I used lenses and prisms of rock-salt; with the two others glass lenses, and prisms of flint glass or of rock-salt. In the experiments in which Bourbouze's lamp was used, I modified the radiation by making it pass through a glass trough full of water interposed between the source and the slit.

It would be impossible to detail all the results of my experiments;

but I will give a comparative view of the results obtained with a beautiful prism of rock-salt, using as a source of heat either the gas-lamp or Bourbouze's lamp.

All the arrangements were the same in the two sets of experiments; in both cases the prism was in the position relative to the minimum deviation of the red, which for the extreme red was 40° 18′. Under these circumstances, working with the Bourbouze lamp, and taking as the unit of effect that obtained in the extreme red, that obtained at half a degree from this position is 2·2, at 1 degree 0·3 only, and at 1° 25′ it is zero. At the same time the rays of the first three layers are transmitted through a fluor-spar trough containing a layer of water 2 millims. in thickness, in the proportions of 0·90, 0·60, and 0·75.

On the other hand, with a bat's-wing burner, taking as unit the effect produced in the extreme red, that obtained at half a degree from this position becomes 4 instead of 2·2, at 1 degree it is 5 instead of 0·3, and at 2 degrees it is still very appreciable. The spectrum thus extends much further into the obscure region. But it is far less transmissible through water. For the band at half a degree from the obscure red the transmission is scarcely 0·14 instead of 0·60, and for that at a distance of 1 degree from the red it becomes insignificant.

Other differences are met with between the spectra furnished by these two sources. With the gas-burner, under the conditions of my experiments, no heat is found either in the yellow or the green, and still less in the extreme white of the spectrum. With Bourbouze's lamp I easily found some in the green, although the intensity of the maximum was not different in the two cases.

I may also be permitted to adduce the following results.

Working with Bourbouze's lamp, the transmissibility of rays of the maximum through water seemed a little less than that of the rays which precede or succeed them in the order of refrangibility.

A similar effect is observed in the solar rays; I have also observed a similar maximum in investigating the action of a trough full of chloroform on the rays from a gas-burner.

Iodized chloride of carbon allows all the obscure part of the radiation from this source to pass in abundance; in other words, the transmission through it of the extreme red rays is very little different from that of the other obscure rays; if there be any difference, it is in favour of the transmissibility of the least-refrangible rays. The luminous part of the spectrum is reduced by the action of this absorbent to two beautiful bands, one red and the other violet, separated by a well-defined dark space.

The transmissibility through æther diminishes with the refrangibility when a moderator-lamp is used as source of heat; but it is very appreciable for rays of the maximum.

All these experiments agree with those I had the honour of presenting to the Academy the 9th of last August, to prove that if, in pure spectra, we isolate the pencils formed of rays whose deviations by the same prism are almost identical, these pencils may be very unequally transmissible through the same absorbent if they arise from different sources.—*Comptes Rendus*, Nov. 30, 1868.

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AUGUST 1869.

XI. *On the Constants of Capillarity of Molten Bodies.* *By* G. Quincke*.

1. I POINTED out in a previous communication† that the constants of capillarity of different fluids might be compared at temperatures in the immediate neighbourhood of their solidification- or melting-points. I have now thought it proper to extend the determinations given elsewhere to a greater number of chemical elements and compounds, as the forces which the particles of any given fluid exert upon each other certainly depend on circumstances less complicated than those between particles of heterogeneous substances, and we may hope accordingly to obtain some clearer ideas in this way of the nature of the perplexing molecular forces, which act (almost always) only at exceedingly small distances.

The following inquiry rests on two principles, previously established by Dr. Thomas Young, which, however, I may, for the sake of connexion, demonstrate in this place.

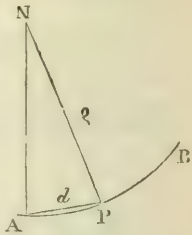
2. A mass of fluid, m, at the point P of the free surface (*i. e.* bounded by vacuum) of a fluid is attracted by another particle A of the surface-layer (fig. 1), from which its distance is d, with

* Communicated in abstract to the Royal Academy of Sciences, Berlin, May 28, 1868. Translated by Professor Jack, Owens College, Manchester.

† *Berliner Monatsbericht*, Feb. 27, 1868, p. 132. Pogg. *Ann.* vol. cxxxiv. p. 356.

the force $mm'\phi(d)$. The direction of this force is the line joining the particles. The function of the distance depends on the resultant of attracting and repelling forces, and disappears when d is larger than the radius of their sphere of action, which is a barely sensible magnitude. The plane through A and the normal at P to the fluid surface, cuts the latter in a curve which, near P, coincides with a circle whose radius is ρ.

Fig. 1.



A second particle, m_1, symmetrically situated at B on the other side, exerts the same force as A. The components of these two forces perpendicular to the normal destroy one another; the sum of the components parallel to the normal, which is the resultant of the two forces, is

$$2mm_1\phi(d)\cos(r, d) = mm_1\phi(d)\,\frac{d}{\rho}.$$

We obtain the action of all the molecules of the normal section on the particle m situated at P by summing up these expressions from $d=0$ to $d=$ a certain value exceeding the indefinitely small radius of the sphere of molecular action. Neglecting the constant, we have for this sum

$$\frac{1}{\rho}\Sigma mm'\phi(d)\,.\,d = \frac{h}{\rho}.$$

Calling ρ_1 the radius of curvature of a second normal section which is perpendicular to the former, similar considerations give a similar result, and the whole action of the particles in two normal sections perpendicular to each other on a particle at the point P is

$$k + h\left(\frac{1}{\rho} + \frac{1}{\rho_1}\right),$$

where k is the attraction which the particles of two normal sections perpendicular to each other exert on an element of the plane surface of the size of the unit surface. The well-known principle of Euler gives

$$\frac{1}{\rho} + \frac{1}{\rho_1} = \frac{1}{R} + \frac{1}{R_1} = \text{constant},$$

where R is the greatest and R_1 the least radius of curvature on the surface. The entire action of the mass of fluid on P, or the capillary pressure (p) at the point P of the fluid-surface, is therefore

$$p = \Sigma k + \left(\frac{1}{R} + \frac{1}{R_1}\right)\Sigma\,.\,h,$$

or, introducing two new constants for these summations,

$$p = K + \frac{H}{2}\left(\frac{1}{R} + \frac{1}{R_1}\right). \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This pressure is normal to the surface. K is the pressure at a point on the plane fluid-surface, H is the difference of pressures which would be exerted on the unit of a plane fluid-surface and on the unit surface of a sphere with unit radius. The right-hand term of (1) may become negative if the two radii of curvature lie outside the fluid, or when the surface is concave. Both H and K depend only on the nature of the fluid; both stand for the constants which Laplace* denoted by the same letters. The constants h and k are proportional to the masses which exert influence. If the density of the fluid be the same inside and on the surface and be called ϵ, k and h (and therefore also K and H) must be proportional to ϵ^2 for the same values of ϕ and the same values of the radius of the sphere of activity. Accordingly, assuming an increasing temperature and taking ϕ as constant, the capillary pressure must decrease proportionally to the square of the density.

Experiment teaches that (1) is true for points in the free surface not only in presence of vacuum, but also when that surface is bounded by any gas or by atmospheric air.

3. If z be the elevation of a point P in a capillary surface above the level or horizontal part of the surface, we deduce from (1), and from the hydrostatical principle that there must be the same pressure throughout a horizontal plane within the fluid,

$$\mathrm{M}gz = \frac{\mathrm{H}}{2}\left(\frac{1}{\mathrm{R}} + \frac{1}{\mathrm{R}_1}\right), \quad . \quad . \quad . \quad . \quad . \quad (2)$$

in which M is the mass of a unit volume of a fluid, and g the accelerating force of gravity. For surfaces of rotation and points at distance x from the axis of rotation we have, therefore,

$$z = \tfrac{1}{2}\frac{\mathrm{H}}{\mathrm{M}g} \cdot \frac{1}{x}\frac{d}{dx}\frac{x\frac{dz}{dx}}{\left(1+\frac{dz^2}{dx^2}\right)^{\frac{1}{2}}}. \quad . \quad . \quad . \quad . \quad (3)$$

If a hollow cylinder, the radius of which is r, be immersed in a fluid with a level surface, and if the axis of z be its axis, the volume between the two cylinders which have z for their height above the level, and x and $x+dx$ for the radii, will be $z \,.\, 2\pi x\, dx$; and the entire weight W of the fluid which is raised above the level is

$$\mathrm{W} = \mathrm{M}g\int_0^r z \,.\, 2\pi x\, dx; \quad . \quad . \quad . \quad . \quad . \quad (4)$$

or, substituting the value of z given in (3),

$$\mathrm{W} = \mathrm{H}\pi\int_0^r \cdot \frac{d}{dx} \cdot \frac{x\frac{dz}{dx}}{\left(1+\frac{dz^2}{dx^2}\right)^{\frac{1}{2}}} \cdot dx = 2\pi r \cdot \frac{\mathrm{H}}{2}\left(\frac{\frac{dz}{dx}}{\left(1+\frac{dz^2}{dx^2}\right)^{\frac{1}{2}}}\right)_{x=r}. \quad . \quad (5)$$

* *Œuvres de Laplace*, vol. iv. p. 407 (1845).

If we call ω the angle which the last element of the fluid surface, where it meets the solid, makes with the vertical solid bounding wall,

$$\left(\frac{dz}{dx}\right)_{x=r}=\cot\omega\,.\left(\frac{\dfrac{dz}{dx}}{\left(1+\dfrac{dz^2}{dx^2}\right)^{\frac{1}{2}}}\right)_{x=r}=\cos\omega\,;$$

and equation (4) becomes

$$\frac{W}{2\pi r}=\frac{H}{2}\cos\omega. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The weight of the fluid per unit of length of the circumference of the cylinder which is lifted above the horizontal level is $\frac{H}{2}\cos\omega$; *i. e.* it is independent of the radius of the cylinder, and depends only on the nature of the fluid and of the enclosing solid wall. The equation is also true for cylinders not hollow; and every vertical wall may be considered approximately a part of such a hollow or solid cylinder.

In fluids which wet the solids (*i. e.* where the last element of the fluid layer is vertical) ω is 0, and

$$\frac{W}{2\pi r}=\frac{H}{2}=\alpha. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

The weight of a fluid sustained per unit of length of the line of contact (which is the line of intersection of the vertical wall and the capillary surface) *is a constant quantity, and measures the mutual attraction of the particles of the given fluid—that is, is its cohesion- or capillarity-constant.*

Since Poisson's time, the quantity

$$a^2=\frac{H}{Mg}=\frac{2\alpha}{Mg} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

is frequently called the constant of capillarity. The advantage is, that when it is divided by the inner radius it gives the mean elevation above the horizontal level to which a fluid which wets the solid ascends. The elevation of a fluid which wets a plane vertical wall, or the rise of the highest point of the curved fluid surface over the horizontal level, is α.

4. Equation (7) is true also for drops which are formed at the mouth of a vertical pipe, on the assumption that, in consequence of the gradual accession of new fluid, the same pressure is found in the interior fluid, at the mouth of the pipe, as in a level fluid surface. The drop goes on increasing till $\omega=0$, or till the highest element of the fluid is vertical, and then it falls off. If the radius of the cylinder on which the drop is formed be very small, the weight of the portion of fluid which remains hanging

may be neglected, and the weight of the portion of the drop which falls may be treated as the W in equation (7).

We may equally neglect the fact that new fluid comes down at the time when the drop is separating, which tends to make the drop too large. When this access of fluid is too great, on the other hand, there is a thin jet of fluid which may readily be resolved into smaller drops by taps from the outside. This is the explanation of the fact that, in the case of many fluids, the drops attain a maximum for a determined velocity in the supply of the issuing fluid*.

Although it thus appears that the process of the formation of drops is exceedingly complicated, the application of equation (7) would give us approximate values of the capillarity-constants α; and this method has at least the recommendation that there is no better, or none which is not complicated by too many experimental difficulties.

5. The experiment is simplest for *gold* and *silver*. Vertical threads of these metals were held by pincers and brought down into a small gas-flame the dimensions of which were not greater than 3 millims. diameter and 8 millims. height, so that the metal, as soon as it was melted, formed in a drop at the lower end of the thread. The drop increased in this way, and rose on the solid thread, which was gradually lowered to the flame. When it was too large it fell into a vessel filled with water, and was immediately solidified, and afterwards dried and weighed. After a little practice it became easy to avoid any shaking of the threads, by which the drops were apt to be too soon detached.

The molten metal was colder above than below; and at the upper part the temperature was only a little above that of the melting-point of the substance. The weight of the drop in milligrammes, divided by the circumference of the wire in millimetres, gives us accordingly the constant of capillarity α for that melting-point.

The shorter the distance between the drop and the pincers holding the wire, the larger the drops seemed to be. This was due probably to the lower temperature of the drop, in consequence of the abstraction of heat by the wire and pincers.

Further, the drops from a gold wire melted over a common gas-flame and over one fed with oxygen weighed nearly the same; so that the influence of temperature in these experiments may be neglected.

The diameter of the wires was measured by a microscope and an eyepiece-micrometer which gave one hundred divisions. Each single division (and tenths of a division could easily be estimated) corresponded, therefore, according to the magnifying-power used, to from ·007 millim. to ·02 millim.

* Compare Pogg. *Ann.* vol. cxxxi. p. 130.

The silver was stated to be chemically pure; the gold was slightly alloyed with silver, chiefly to facilitate the process of wire-drawing.

Glass threads, drawn out before the lamp from a thicker piece of glass, were also treated like wires. The determinations, however, were less trustworthy, because glass becomes soft before melting, and accordingly, through a commencing drop-formation above the fluid drop, the glass cylinder from which the drop falls off is really widened. A series of determinations was made for each wire, and the mean of them taken. The results collected below prove that the weight of the drops really increases (as it ought to do according to theory) in proportion to the diameter of the wires.

Silver.			Gold.			Glass.		
$2r$.	W.	α.	$2r$.	W.	α.	$2r$.	W.	α.
millim.	grm.	mgrms.	millim.	grm.	mgrms.	millim.	grm.	mgrms.
0·4971	0·0733	47·14	0·2566	0·080	99·24	0·6709	0·0422	20·02
0·2318	0·0299	41·13	0·2009	0·075	103	0·5232	0·0273	16·62
0·0993	0·0130	41·66	0·0695	0·0215	98·42	0·2441	0·0134	17·48
0·0775	0·0110	41·09	......	......	......	0·2006	0·0115	18·24
	Mean	42·75	......	Mean	100·22	......	Mean	18·09

6. The measurements for *platinum* and *palladium* wires were made in the same way as those for gold and silver. Oxygen, however, was conducted into the gas-flame through a platinum nozzle. Palladium was volatilized with such remarkable rapidity in the oxyhydrogen flame, that I might compare the palladium drops in this respect to ether drops at the ordinary temperature. The melting- and boiling-points appear to be very near each other, since I was unable with an ordinary blowpipe-flame (the pointed flame of the glass-blowers) to melt the metal; the drop lost more by volatilization, as soon as it had attained a certain size, than it gained by fusion of new wire. Accordingly I found the values of α always too small in my numerous experiments, and that which I give below makes no pretence to accuracy. When palladium solidifies, there are formed on the smooth drop-surface needle-shaped excrescences, which give the mass a peculiar appearance.

Platinum.			Palladium.		
$2r$.	W.	α.	$2r$.	W.	α.
millim.	grm.	mgrms.	millim.	grm.	mgrms.
0·5675	0·2912	163	0·6829	0·1300	163·4
0·3689	0·2655	177·4			
0·1921	0·0996	165·1			
0·0993	0·0530	169·8			
0·0767	0·0410	169·9			
	Mean	169·04			

7. To obtain drops of *tin* and *selenium*, these substances were molten in glass tubes, the lower part of which was funnel-shaped, ending in a thin vertical pipe. The part of this pipette-shaped pipe which was cut off by the glass-knife was used to determine the inner or outer diameter by means of microscope and eyepiece-micrometer. Figs. 2 and 3 show the drop attached to the outer and inner circumference. Determinations in which the drops had formed partly on the inside, partly on the outside (fig. 4),

Fig. 2.

Fig. 3.

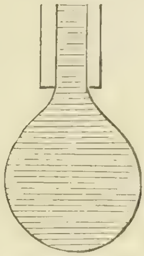
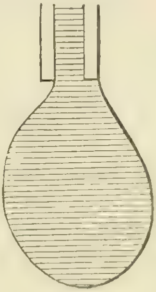
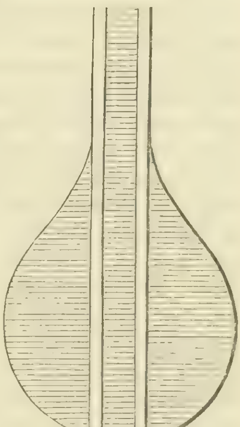


Fig. 4.



or where the outer glass wall was wetted by the drops (fig. 5), were rejected. The drops fell into a flat porcelain saucer filled with water, or which was simply kept cold. I took great pains to see that the drops were formed as slowly as possible. They followed each other usually so much the more slowly the more the cooling down progressed. The last drop which fell before complete solidification was heavier than that preceding, which was again heavier than that before it, and so on; so that the capillarity-constant increases with diminishing temperature. The difference, however, is insignificant, and in some cases I have given means collected from these last drops. Strictly speaking, the last drop determines the capillarity-constant in the neighbourhood of the melting-point. From the upper part of the pipette-shaped vessel a piece of india-rubber tubing went to the mouth, which made it easy to regulate the speed of the issuing fluid.

Fig. 5.



The determinations for *zinc* were made in the same way; but, in consequence of the higher melting-point, it was found more

convenient to use the glass-blower's flame instead of that of an ordinary Bunsen.

Selenium.			Tin.			Zinc.		
$2r$.	W.	α.	$2r$.	W.	α.	$2r$.	W.	α.
millim.	grm.	mgrms.	millim.	grm.	mgrms.	millim.	grm.	mgrms.
0·9670	0·0214	7·045	0·665	0·1200	57·41	0·8368	0·2122	80·74
0·7164	0·0158	7·021	0·642	0·1245	61·69	0·7285	0·1920	83·90
0·6688	0·0155	7·377	0·549	0·0976	56·52	0·7020	0·1847	83·75
0·6125	0·0140	7·276	0·470	0·0800	54·25			
			0·437	0·090	65·39			
			0·395	0·072	58·08			
			0·311	0·064	65·62			
	Mean	7·180	......	Mean	59·85	......	Mean	82·79

8. In the case of bodies which, like *phosphorus, cadmium, lead, antimony, bismuth,* oxidize easily, it was necessary to produce the drops in an atmosphere of carbonic acid. In the case of *zinc* also, where oxidation might have been suspected in the open air, several of the experiments were performed in an atmosphere of carbonic acid, which demonstrated that the capillarity-constants are little, if at all, dependent on the nature of the surrounding gas when the surface is not altered by oxidation.

The phosphorus was melted in a test-tube under water, a ball of india-rubber fastened on the glass tube which had been drawn out into the shape of a pipette, and the molten phosphorus sucked up by pressure on this ball. The glass pipe was carefully dried on the outside with blotting-paper. In these experiments it often happens that the phosphorus remains in a fluid state far below its melting-point, and that we find the weight of the drop or the capillarity-constant too large. Possibly the abnormal result given by Dupré*, who found $\alpha=8{\cdot}407$ milligrammes for 46° C., a number about twice as large as that which is deduced from my experiments, is to be explained in this way. The drops taken up under water remain also fluid for a considerable time; and it happens frequently, when they follow each other quickly, that several gather themselves into one, which then itself continues fluid for a considerable time. In the determination of the constant of capillarity it is natural in this case to take account of the number of drops which have been collected into one.

Zinc and antimony were molten in the flame of the glass-blower's lamp, cadmium and lead in that of a Bunsen's burner.

The carbonic acid was obtained from marble and hydrochloric acid, led through a washing-bottle with a solution of carbonate of soda and a series of Babo's bulb-tubes, which were also

* *Ann. de Chim. et de Phys.* vol. ix. (1866) pp. 330 & 384.

wet with this solution, so as to remove the last traces of hydrochloric acid which might be taken over along with it. A black caoutchouc tube and a vertical glass pipe conducted the carbonic acid to the bottom of a beaker filled with water to the height of several centimetres, over the edge of which the gas then escaped. The lower opening of the pipette tubes was brought into this atmosphere of carbonic acid; and care was taken, by moving them about, that the different solidified drops in the beaker-glass should fall at different places on the bottom.

In the cases of cadmium and phosphorus, which are very readily oxidized, this arrangement was frequently unsatisfactory. The carbonic acid was in this case led into the lower end of a glass tube, A B, of 120 millims. height and 20 millims. diameter, which dipped into a saucer of porcelain filled 15 millims. high with water. The narrow glass tube was completely filled with pure carbonic acid; a slow current of gas prevented its being mixed by diffusion with atmospheric air. Care was taken, by shifting the porcelain saucer under the fixed glass tube in the middle of which the drops formed, that the single drop should solidify at different places.

The formation of drops in the different substances takes place in different, and frequently in highly characteristic ways. A mere glance at the solidified drop is sufficient to decide from which of the metals it has been formed.

Cadmium exhibits a remarkable phenomenon when the carbonic-acid atmosphere contains traces of air. A long jet of molten metal then falls out of the opening of the glass tube, and speedily becomes tarnished at different points. These spots show clear thin cracks parallel to the axis of the cylindrical jet. This swells regularly out at certain places, where a formation of drops takes place, while the metal surface appears as if oxidized.

Zinc (in CO^2).			Lead (in CO^2).		
$2r$.	W.	α.	$2r$.	W.	α.
millim.	grm.	mgrms.	millim.	grm.	mgrms.
0·9949	0·2550	81·58	0·579	0·0840	46·17
0·8756	0·2620	95·23	0·543	0·0754	44·20
0·7668	0·2225	92·37	0·288	0·0422	46·61
0·7534	0·1930	81·56			
	Mean	87·68	......	Mean	45·66
Phosphorus (in CO^2).			Antimony (in CO^2).		
0·8882	0·0127	4·559	1·308	0·1040	25·30
0·8497	0·0123	4·610	0·9224	0·0724	24·99
0·8024	0·0115	4·562	0·6357	0·0480	24·48
0·5325	0·0092	3·471			
0·5066	0·0060	3·770			
	Mean	4·194		Mean	24·92

Bismuth (in CO^2).		
$2r$.	W.	α.
millim.	grm.	mgrms.
0·8650	0·1150	42·31
0·5265	0·0598	36·16
0·4238	0·0500	37·57
0·3609	0·0450	39·69
	Mean	38·93

9. The same process as was applied in the case of phosphorus was extended to sodium and potassium, with this difference, that the water was replaced by petroleum, in which the drops were received, and that the carbonic acid, after its treatment with carbonate of soda, was thoroughly dried in a wash-bottle and Babo's bulb-tubes by means of hydrated sulphuric acid. The determinations present great difficulties in the case of these metals, since, whenever the carbonic acid is not quite pure or the temperature is a little high, we have oxidation of the surface and phenomena like those with cadmium. It may even happen in the latter case that the metal kindles in the carbonic acid. I have accordingly not been able for potassium to make determinations free from error, since a slight oxidation of the surface seems to take place even under favourable circumstances. The mouth of the glass tube is readily lined with a border of solid potash, the diameter $2r$ becomes greater than the measurement gave it, and the constant of capillarity found is too great. Accordingly I give the numbers for potassium subject to these reserves. The potassium-drops are readily distinguished from all others by their great volume.

Sodium (in CO^2).			Potassium (in CO^2).		
$2r$.	W.	α.	$2r$.	W.	α.
millim.	grm.	mgrms.	millim.	grm.	mgrms.
1·384	0·1166	26·83	1·195	0·1552	41·35
0·9426	0·0780	26·34	1·105	0·1444	41·58
0·9224	0·0785	27·10	0·9629	0·1095	36·21
0·895	0·0665	23·65	0·8295	0·0889	34·12
0·597	0·0465	24·85	0·7756	0·0840	34·47
			0·6766	0·0790	37·17
			0·2218	0·0242	34·73
	Mean	25·75	......	Mean	37·09

10. For a series of salts, beads of the substances on a horizontal platinum wire of measured diameter were molten in a small gas-flame or in the blowpipe-flame of the glass-blowers. More of the salt could be added or taken away, as was required,

by a second platinum wire. By trial, the quantity of salt was determined which was just carried by the lower end of the vertical wire, or which just fell off when it was a little too heavy or when the temperature was too high. A horizontal wire carries a larger drop than a vertical one in the same circumstances, in consequence of the longer line of contact.

In many cases, the bead ascends on the vertical wire, because it cools from the top downwards; and the meniscus always tends towards the position where the capillarity-constant α of the molten salt has its greatest value.

The beads of salts which fell off were taken up on a piece of platinum-foil and weighed, in the case of hygroscopic salts, between two watch-glasses. The measurements given below are the means of the drops of the salt which fell off, and of those which still adhered to the platinum wire.

	Boracic acid.		Borax.		Phosphorus-salt.	
$2r$.	W.	α.	W.	α.	W.	α.
millim.	grm.	mgrms.	grm.	mgrms.	grm.	mgrms.
0·5569	0·01982	10·16	0·03653	20·88	0·0367	20·98
0·3974	0·0132	10·56	0·0285	22·83	0·02472	19·81
0·1808	0·00577	11·33	0·01198	21·08	0·01188	20·91
	Mean	10·69	Mean	21·60	Mean	20·57
	Carbonate of potash.		Carbonate of soda.		Chloride of calcium.	
0·5569	0·0278	16·38	0·0341	19·49	0·02686	15·36
0·3974	0·02097	16·79	0·02888	23·14	0·01843	14·77
0·1808	0·0093	15·89	0·0115	20·25	0·00898	15·81
	Mean	16·33	Mean	20·96	Mean	15·31
	Chloride of potassium.		Chloride of sodium.		Chloride of lithium.	
0·5569	0·01611	9·208	0·0196	11·20	0·02137	12·22
0·3974	0·01203	9·636	0·01392	11·15	0·01523	12·20
0·1808	0·00551	9·703	0·00712	12·54	0·0067	11·80
	Mean	9·516	Mean	11·63	Mean	12·07

Nitrate of potash.			Chloride of silver.		
$2r$.	W.	α.	$2r$.	W.	α.
millim.	grm.	mgrms.	millim.	grm.	mgrms.
0·5569	0·0170	9·716	0·530	0·0315	18·92
0·3974	0·0130	10·41	0·265	0·0159	19·11
0·1808	0·00553	9·763			
	Mean	9·954	......	Mean	19·01

11. If we treat bromine with pure hydrated sulphuric acid in order to have it perfectly free from water, and if we then allow it to drop in the way described above, the numbers which we obtain in this way for the constants of capillarity nearly agree with those determined by Bède*, who found $a^2=2{\cdot}51$ square millims. to $2{\cdot}81$ square millims. for $6^\circ{\cdot}8$ C.

Bromine ($\sigma=3{\cdot}18$).			
$2r$.	W.	α.	$\alpha=\frac{2\alpha}{\sigma}$.
millim.	grm.	mgrms.	sq. millims.
0·8407	0·0088	3·335	2·097
0·6998	0·00744	3·385	2·128
0·4934	0·0053	3·420	2·151
Temp. =13° C.			

The observation of the elevation h in capillary tubes of radius r for the same substance gave

Bromine.		
$2r$.	h.	$a^2=hr$.
millim.	millims.	millims.
0·4635	11·7	2·712
0·2079	28·3	2·943
Temp.=13° C.		

These numbers agree, all of them, very closely with the determinations by Bède.

I suspected, however, that the capillarity-constants of bromine altered rapidly with the temperature, and I attempted accordingly to make a determination in the neighbourhood of the freezing-point. A rather wide test-tube, which contained bromine and a glass tube divided into millimetres, of ·208 millim. diameter, was set obliquely in a mixture of salt and snow, so that the bromine solidified. The glass tube was then brought out of the freezing-mixture into the usual temperature and set vertical. When the bromine was thawed, it showed an elevation of 37·5 millims., from which we deduce

$$a^2=3{\cdot}895 \text{ sq. millims.,} \quad \alpha=6{\cdot}328 \text{ milligrms.}$$

for the melting-point of bromine.

Determinations of this sort are made much more easily in winter.

* *Mém. Cour. et des Savants etrangèrs de l'Académie de Belgique*, vol. xxx. (1860) p. 163.

12. I was interested to note the values of the constants of capillarity for water and mercury by this method of drops, as I had made numerous determinations of them at the usual temperatures by other methods. As the following Tables show, the results gave very different values (especially for water) for the constants, according to the speed with which the drops were formed.

The numbers obtained for mercury are usually smaller than those resulting from my own previous determinations* (from 55·21 to 58·79 milligrammes); but they differ from the results of other observers, which were obtained by much more accurate methods (elevations in tubes, and drops on a horizontal surface) by inconsiderable amounts. According as the measurements were made immediately after the formation of the surfaces or at a later time, the results for α ranged for mercury between 40 and 50 milligrammes.

Mercury.				
	Slow.		Quick.	
$2r$.	W.	α.	W.	α.
millims.	grm.	mgrms.	grm.	mgrms.
1·029	0·1307	40·42	0·1359	42·06
0·308	0·0422	43·62	0·0522	57·06
Water.				
2·478	0·0422	5·428	0·0450	5·781
0·493	0·0097	6·259	0·0170	10·96

13. By observation of the capillary elevations, the following results were found for water:—

Gay-Lussac† . . . $\alpha = 7·565$
Hagen‡ . . . . $= 7·558$

By weighing the capillary-raised meniscus,

Wilhelmy§ . . . $= 7·945$

I had myself determined the capillarity-constant for water ten years ago, when I observed the elevation on the inner wall of a thin-walled glass cylinder of about 50 millims. diameter.

Distilled water was boiled in a glass vessel with vertical walls till about two-thirds of the fluid was left. While the water was still boiling the vessel was corked, and a glass rod with

* Pogg. *Ann.* vol. cv. p. 33 (1858).
† Poisson, *Nouvelle Théorie de l'Action Capillaire,* p. 113 (1831).
‡ *Abh. d. Berl. Akad.* 1845, p. 34.
§ Pogg. *Ann.* vol. cxix. p. 186 (1863).

a fine point was inserted air-tight in the cork. When the bottle was inverted, after the water had cooled down with the access of the external air shut off, this point lay in the water immediately below the lowest part of the upper surface of the fluid. The meniscus was formed on a part of the glass wall which had previously remained wet, and was observed with the microscope of the cathetometer which I previously described*. One or two seconds after the formation of the meniscus, the cross threads of the microscope were directed upon its upper limiting surface. This surface, lighted up from behind by a paper half white, half black, so that the horizontal limit of the black and white lay at the same height with it, appeared as a well-defined dark line. The point lying in the water was mirrored in the water-surface bounded by the space free from air. The cross threads were set in the middle between the point and its image, and so the position of the horizontal part of the fluid surface was determined. The point seen through the water distinctly in the microscope appeared in the middle of the bottle, while the wall of the meniscus was first seen distinctly sideways from the middle of the bottle; between the two determinations the microscope had therefore to be shifted on the plate of the cathetometer, which was made accurately horizontal by a spirit-level.

Previously to this boiling, the bottles as they came from the glass-blower were purified partly with water and alcohol, partly with hot concentrated sulphuric acid, and were then filled with distilled water and allowed to stand aside for some hours. The water used for the experiments was put in a little before the boiling. The measurements were made as soon after the turning upside down of the bottle as possible, as the elevation diminishes, at first rapidly and then gradually, immediately after the formation of the capillary meniscus.

I may give here a series of determinations for different bottles the diameters of which are denoted by D, from which we may infer the accuracy which it is possible to secure in such determinations.

	I.	II.	III.	IV.
	millims.	millims.	millims.	millims.
	D=52·7	52·05	50·3	50·3
Temp.	17°·1 C.	17°·5	17°	(17° ?)
	millims.	millims.	millims.	millims.
	4·015	4·072	4·004	4·207
	4·176	4·015	4·105	4·164
	4·110	3·993	4·180	4·110
	4·155	3·900	4·160	4·166
	....	....	4·146	....
Mean .	4·114	3·995	4·119	4·161

* Pogg. *Ann.* vol. cv. p. 12 (1858).

	V.	VI.	VII.	VIII.
	millims.	millims.	millims.	millims.
D=	50·3	50·3	50·3	
	(17° ?)	19°·44	19°·8	
	millims.	millims.	millims.	millims.
	4·345	4·033	4·016	4·047
	4·232	4·024	4·036	4·048
	4·284	4·040	4·000	4·047
Mean .	4·287	4·032	4·013	4·047

Between the series VII. and VIII. of these experiments the water, which had been previously purged of air, was shaken about and mixed with it, so that, within the limits of observation-error, water void of air and ordinary water seem to have the same constant of capillarity.

In the case of water which had stood in the bottles for several weeks shut off from the air, we had:—

	IX.	X.	XI.
	millims.	millims.	millims.
D=	52·7	50·3	50·3
Temp.	13°·1	17°·48	18°·05
	millims.	millims.	millims.
	3·922	3·910	4·075
	3·900	3·868	4·025
	3·899	3·824	4·007
Mean .	3·907	3·867	4·036

If the water is left some time in the same glass vessel, the elevation or constant of capillarity of the water appears, according to these experiments, to diminish. I imagine that this has its explanation partly in the solution of the glass wall in the distilled water.

If we neglect the curvature of the walls, the elevation is precisely the constant a which was determined by Poisson. Assuming the increase of the capillarity-constant given by Brunner* for a diminution of temperature, we have thus for water, at 0°,

$$a=4{\cdot}193 \text{ millims.}, \quad a^2=17{\cdot}58 \text{ sq. millims.}, \quad \alpha=8{\cdot}79 \text{ milligrms.}$$

The values of the constants are greater than those found by other observers, because they allowed a longer interval to elapse between the formation and the measurement of the meniscus.

14. The different values of the constant α are collected in the following Table, and the substances are arranged according to its magnitude. It must be understood that, except where I have expressly mentioned the fact, all the substances were as pure as I could obtain in commerce.

* Pogg. *Ann.* vol. lxx. p. 515 (1847).

The constant for sulphur is calculated from the experiments of Frankenheim†, and that for wax from those of Wertheim‡.

Besides the constant α, the Table gives $a^2=\frac{2\alpha}{\sigma}$ and a, where σ is the specific gravity of the fluid. I have determined the specific gravity σ_0 for the temperature 0°, with the exception of those cases which are marked *; and I have calculated σ for the melting-point. The values of σ which are given in these cases are more or less unreliable, on account of our ignorance of the expansion-coefficients, the precise melting-points, and the expansion or contraction which takes place during the process of melting.

Table I.

Capillarity-constants of Molten Bodies.

Substances.	Melting-point.	σ_0.	σ.	α.	a^2.	a.
	°			mgrms.	sq. mill.	millims.
1. Platinum..............	(2000)	20·033	18·915	169·04	17·86	4·227
2. Palladium	(1950)	11·4 *	10·8	(136·4)	25·26	5·026
3. Gold	1200	18·002	17·099	100·22	11·71	3·423
4. Zinc (in CO^2).........	360	7·119	6·900	87·68	25·42	5·042
5. Zinc (in air)	360	7·119	6·900	82·79	24	4·899
6. Cadmium (in CO^2)...	320	8·627	8·394	70·65	16·84	4·103
7. Tin	230	7·267	7·144	59·85	16·75	4·091
8. Mercury	−40	13·596	......	58·79	8·646	2·941
9. Lead (in CO^2).........	330	11·266	10·952	45·66	8·339	2·887
10. Silver	1000	10·621	10·002	42·75	8·549	2·923
11. Bismuth (in CO^2)	265	9·819	9·709	38·93	8·019	2·831
12. Potassium (in CO^2)...	58	0·865*	......	(37·09)	85·74	8·768
13. Sodium (in CO^2)	90	0·972	......	25·75	52·97	7·278
14. Antimony (in CO^2)...	432	6·620	6·528	24·92	7·635	2·764
15. Borax	(1000)	2·6 *	2·5	21·60	17·28	4·254
16. Carbonate of soda....	(1000)	2·509*	2·45	20·96	17·11	4·136
17. Microcosmic salt	......	2·502	2·45	20·57	16·79	4·098
18. Chloride of silver	......	5·55 *	5·5	19·01	6·911	2·629
19. Glass	(1100)	2·452	2·380	18·09	15·21	3·899
20. Carbonate of potash ..	1200	2·300	2·2	16·33	14·82	3·846
21. Chloride of calcium....	......	2·219	2·15	15·31	14·24	3·774
22. Chloride of lithium....	......	1·998*	......	12·07	12·10	3·478
23. Chloride of sodium....	......	2·092	2·04	11·63	11·40	3·377
24. Boracic acid	(1300)	1·83 *	1·75	10·69	12·22	3·495
25. Nitrate of potash......	339	2·059	2·04	9·954	9·759	3·124
26. Chloride of potassium.	......	1·932	1·870	9·516	10·18	3·19
27. Water	0	1	......	8·79	17·58	4·193
28. Selenium	217	4·3 *	4·2	7·180	3·419	1·849
29. Bromine	−21	3·187	3·25	6·328	3·895	1·973
30. Sulphur	111	2·033	1·966	4·207	4·280	2·068
31. Phosphorus (in CO^2).	43	1·986*	1·833	4·194	4·575	2·140
32. Wax....................	68	0·963	......	3·40	7·061	2·657

† Pogg. *Ann.* vol. lxxii. p. 193 (1847).
‡ *Comptes Rendus*, 1857, vol. xliv. p. 1021.

From the foregoing Table the remarkable result follows, that the values of the constant a^2 arrange themselves for the metals, and partly also for the other substances, in groups in which they are nearly equal, and which are separated from each other by the fact that it is a different whole multiple of 4·3 in each.

TABLE II.

I. $a^2=4·3$.		II. $a^2=8·6$.		III. $a^2=12·9$.	
Selenium	3·42	Mercury	8·65	Gold	11·71
Bromine	3·90	Lead	8·34	Chloride of lithium	12·10
Sulphur	4·28	Silver	8·55	Chloride of sodium	11·40
Phosphorus	4·58	Bismuth	8·02	Boracic acid	12·22
		Antimony	7·63		
		Wax	7·06		

IV. $a^2=17·2$.		VI. $a^2=25·8$.		XII. $a^2=51·6$.		XX. $a^2=86$.	
Platinum	17·86	Palladium.	25·26	Sodium.	52·97	Potassium	85·74
Cadmium	16·84	Zinc	25·41				
Tin	16·75						
Borax	17·28						
Carbonate of soda	17·11						
Microcosmic salt	16·79						
Glass	15·51						
Carbonate of potash	14·82						
Chloride of calcium.	14·24						
Water	17·58						

In the foregoing tabular comparison all the substances of the first Table appear except chloride of potassium, nitrate of potash, and chloride of silver, which perhaps ought to be reckoned in group II.,—since these salts, like the other chlorides, have undergone decomposition in melting; and the amount of carbonate may have produced an alteration in the value of a^2, as the surface-layer, which determines the magnitude of the drop, must be the first which is altered. More accurate determinations on the point require more experimental facilities and more complicated apparatus than I at present possess.

Since the volume of the drops which fall from a tube of 2 millims. circumference is a^2, the following law would result from the Tables:—"The volumes of drops of different substances in a state of fusion, at a temperature near that of fusion, falling from tubes of the same diameter, stand to one another in the proportions of 1, 2, 3, &c."

This law is, as might be expected, only approximate, like many physical laws. If we reflect, however, what are the diffi-

culties and sources of error in these observations, that the velocity of the formation of the drop is left out of account, that for a substance such as water, the values of a^2 given by different experienced observers vary between 13·5 and 17·58 sq. millims., we see that the deviations from the law may very possibly be due to errors of observation*. No relation such as has been suspected to exist† between these constants of capillarity and other physical or chemical properties of the substances can be deduced from these Tables.

15. According to equation (7), the constant α is the weight in milligrammes of the mass of fluid which can be carried by 1 millim. of the contact-line of the fluid meniscus, and which is half of the constant which Laplace calls H. The magnitude α measures, therefore, the difference of the forces of pressure which are exerted on the unit of a plane fluid surface, and on the unit of a spherical surface, of diameter 1, in the direction of the normals. We may say, therefore, that α measures the attraction which the particles within the fluid exert for a given form of the surface on a volume of the surface-layer, the base of which is the unit of surface.

If the surface-layer of fluid had the same density in all points with the fluid inside at a finite distance from the surface, the quantity $\frac{\alpha}{\sigma}$, or half the quantity a^2, would measure the attraction which is exerted on the unit mass of the surface by the particles within the fluid, and the attractive functions for different substances would be complete multiples of the same magnitude.

The assumption that in the surface there is the same density as in the inside of the fluid appears, however, not to be admissible. If we conceive the fluid divided into three (partial) layers parallel to the fluid surface, each of these (partial) layers will be attracted by the molecular forces inward, and with a force so much the less the nearer it lies to the centre of the fluid. But the layers overlying each individual layer press upon it, so that the capillary pressure increases up to a certain value, if we proceed inwards from the first external layer. Since the fluid is not incompressible, the density in each separate layer will be different on account of the difference of pressure, and within the fluid it will be different from what it is on the outside. The

* The constants a^2 are determined for a series of simple fluids, *e. g.* ether (5 sq. millims.), alcohol (5·861 sq. millims.), and oil of turpentine (6·708 sq. millims.), for the usual temperature, which is much higher than that of fusion of the fluid. It appears to me, therefore, very probable that these fluids belong to group II., and that, could we determine their capillarity-constants in the neighbourhood of the points of fusion, we should have $a^2=8·6$ sq. millims.

† *Conf.* Dupré, *Ann. der Chem.* (4) vol. ix. p. 330 *et seqq.*

density in the single (partial) layers of the entire surface-layer is therefore not constant at all points, and we cannot assign to a^2 the meaning given above. The following appears to me to represent most accurately the present state of knowledge on the subject.

Taking the radius of the sphere of action as equal for all substances, which makes the volume V of the inside fluid particles which work on the particles at the surface the same, $\frac{\alpha}{\sigma}$ measures the force which the mass 1, uniformly distributed over the volume V, exercises on the surface-layer of the fluid. In other words, the half of Poisson's constant $a^2=\frac{2\alpha}{\sigma}$ measures the attraction which is exerted on a portion of the surface-layer of the fluid, the base of which is unity, by a mass 1 inside the fluid, and it may be called *specific capillary attraction or specific cohesion.*

From the preceding Tables it follows that the *specific cohesion of the metals and many other substances in a molten condition, at temperatures little above their melting-points, is nearly as the numbers* 1, 2, 3, *&c.*

The law expressed in the preceding statement as to the specific cohesion of the fluids becomes intelligible if we assume that the molecular function is the same for all bodies, and that in the surface-layer, the density of which is not the same in all its parts, masses are enclosed which, in different substances, bear to each other the proportions of the series of the natural numbers.

Berlin, October 1868.

XII. *On the Descent of a Solid Body on an Inclined Plane when subjected to alternations of Temperature. By* Henry Moseley, *M.A., Canon of Bristol, F.R.S., Instit. Imp. Sc. Paris, Corresp.**

LET AB (fig. 1) be an elementary plate of the solid, and conceive it to be divided into an infinite number of equal elements by planes perpendicular to its length. Let X be a point so taken in it that, if it were divided in X, the thrust necessary to push the part XA up the plane would equal that necessary to push XB down it. Let the element at X be imagined to have its temperature so raised as just to equal this thrust; and let the temperatures of all the elements in XA, beginning from X, be equally raised in succession. Each will thus be dilated more than the one before it, because its dilatation will be opposed by

* Communicated by the Author.

a less resistance; and the displacement of the extremity upwards will equal the sum of these several dilatations. In like manner,

Fig. 1.

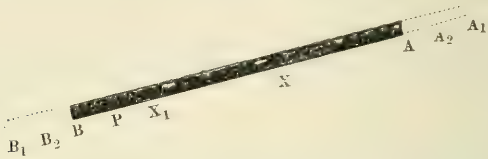


Fig. 2.

if the same temperature be added to the elements of X B in succession, beginning from X, each will be dilated more than the one before it, and the displacement $B B_1$ of the extremity B downwards will equal the sum of these several dilatations. The point X will obviously be nearer to A than to B, because the same thrust of dilatation of the element at X would not be able to push so great a length of the bar up the plane as it would down it.

In this state of the temperature of the plate, let a point X_1 be taken such that, if it were divided there, the strain necessary to pull the part $X_1 A_1$ down the plane would just equal that necessary to pull $X_1 B_1$ up it. Let the temperature of the element at X_1 be so diminished as by its contraction just to produce this strain, and let the temperatures of all the elements from X_1 to A_1 in succession be equally reduced. Each will contract more than the one before it, because a less resistance will be offered to its contraction; and the displacement $A_1 A_2$ of A_1 down the plane will equal the sum of these separate contractions. In the same way the displacement $B_1 B_2$ of B_1 up the plane will equal the sum of the separate contractions of the elements of $X_1 B_1$. The point X_1 will be further from A_1 than B_1, because the same strain of contraction of an element at X_1 would pull a greater length of the bar down the plane than up it. It is by

the dilatation of the greater length of the plate X B favoured by its weight that the extremity B is displaced down the plane when the temperature is raised; whilst it is by the contraction of the less length X_1 B against its weight that it is displaced up the plane when the temperature is lowered. The extremity B is therefore more displaced down the plane by a given raising of the temperature than it is displaced up it by a corresponding lowering. On the whole, therefore, the extremity B is made to *descend* the plane by a given alternation of temperature. It is by the dilatation of the less length X A that the extremity A is displaced up the plane, and by the contraction of the greater length X_1 A_1 that it is displaced down the plane. It is therefore less displaced up by dilatation than it is down by contraction, and on the whole it descends by a given alternation of temperature. Both the extremities A and B of the plate are therefore made to descend when it is subjected to a given elevation and then to a corresponding depression of its temperature; that is, the *whole* plate is made to *descend.*

It is the object of the following paper to discuss the mathematical conditions of this descent with a view to its application to the theory of the descent of glaciers. Formula (22) is the mathematical expression of the result.

Two principal cases arise in this discussion. An increase of temperature has been supposed to be communicated to an element at X such as would be sufficient, if the plate were divided at that point, to push X A up the plane and X B down. This determines the increment of temperature with reference to the length of the plate; and so of the corresponding decrement, which must be sufficient to pull X_1 A_1 down and X_1 B_1 up. The first case is that in which the alternation of temperature to which the plate is subjected is equal to, or greater than, *this.* In this case the plate descends. The second case is that in which the alternation of temperature is less than this. The thrust of dilatation produced by the given increment of temperature of an element at X which is sufficient to push X A up not being sufficient to push X B down, let x B (fig. 2) be a part of the plate which it would be just sufficient to push down, and let the whole plate receive this increment of temperature. The parts X A and x B will then be dilated to A_1 and B_1, but X x will remain undilated. Let now X_1 be a point in the plate at which, if an element experience a corresponding decrease of temperature, the strain of its contraction would be sufficient to pull X A_1 down but not X_1 B_1 up; and let x_1 B_1 be the part of the plate that it would just pull up. If, then, the whole plate experience this decrement of temperature, X_1 A_1 will be contracted to X_1 A_2, and x_1 B_1 to x_1 B_2, but X_1 x_1 will remain uncontracted.

The part $X_1 x$ will thus have remained unmoved either by the increment or the decrement of the temperature, and the plate will not descend. The strain will be greatest on the points X_1 and x_1 when the plate suffers a diminution of temperature; and it is a possible case that the tensile strength may not be sufficient to bear this strain. The plate will then be torn asunder at those points. Although the plate was before too long to be made to descend by the given alternation of temperature, yet the parts into which it is thus separated may not. The points X and x are those which sustain the greatest thrust when the temperature is raised; they are therefore those at which there is the greatest tendency to crush. The distances X A, x B, $X_1 A_1$, $x_1 B_1$ are independent of the length of the plate.

If the plate adhere to the plane, so that, besides the resistance of friction to its descent, there is that of its shearing upon it, and if in any new position *into* which it is sheared the adhesion be supposed to be reestablished as perfectly as it was in the position *from* which it was sheared, and if, lastly, the thrust and strain of expansion and contraction due to an alternation of temperature be sufficient to overcome the resistance to shearing of the surfaces in contact, then for a given weight of the plate and inclination of the plane the resistance to shearing will be the same as it would be if a given addition were made to the resistance of friction; and taking for the coefficient of friction one equal to the sum of the actual coefficients of friction and the coefficient of this equivalent imaginary friction, the cases of friction and adherence may be treated as one of friction only.

Let the plate be rectangular and of uniform thickness, and let it rest lengthwise upon the plane.

Let its dimensions and weight, and the conditions of its dilatation and contraction, be represented as follows:—

a = length in feet at the given temperature T° Fahr.

K = transverse section in square inches.

E* = modulus of elasticity.

λ = dilatation or contraction per foot for every variation of 1° F. in the temperature of the plate or bar.

$l_1 = 1 + \lambda t_1$ = length to which each foot in the length of the bar is dilated when (dilating freely) it is heated from the temperature T° by t_1° F.

$l_2 = 1 - \lambda t_2$ = length to which each linear foot of the bar is shortened, when from the temperature T° it is cooled by t_2°, contracting freely.

* The modulus of elasticity is here assumed to be that weight in pounds which, if applied as a tension to a bar of the metal 1 square inch in section and 1 foot long, would lengthen it by one foot, or which, if applied as a thrust, would (if the same law obtained, *however great* was the compression) compress it by one foot.

$w =$ weight in pounds of a portion of the bar 1 foot long and 1 square inch in section.

$\iota =$ inclination of the plane.

$\phi^{*} =$ limiting angle of resistance between the surface of the bar and the surface of the plane.

$f_1 = \dfrac{\sin(\phi - \iota)}{\cos\phi} =$ thrust per pound of the weight of the bar necessary to push it *down* the plane.

$f_2 = \dfrac{\sin(\phi + \iota)}{\cos\phi} =$ thrust per pound of the weight of the bar necessary to push it *up* the plane.

$x =$ distance in feet of any point P in the bar from the *fixed* end of it.

Case I.—When the *upper* end of the bar is fixed and the lower end free.

$x_1 =$	what x	becomes	when	T° becomes	$(\mathrm{T}^\circ + t_1)$.
$a_1 =$	,, a	,,	,,	,,	
$x_2 =$	,, x	,,	,,		$(\mathrm{T}^\circ - t_2)$.
$a_2 =$	,, a	,,	,,	,,	,,

$\xi_1 =$ value of x in respect to the point where the dilatation of the bar begins.

$\xi_2 =$ value of x in respect to the point where the contraction of the bar begins.

Case II.—When the *lower* end of the bar is fixed and the upper end free.

$\mathrm{X}_1 =$	what x	becomes	when	T° becomes	$(\mathrm{T}^\circ + t_1)$.
$\mathrm{A}_1 =$	,, a	,,	,,	,,	
$\mathrm{X}_2 =$	,, x	,,	,,		$(\mathrm{T}^\circ - t_2)$.
$\mathrm{A}_2 =$	,, a	,,	,,	,,	,,

$\Xi_1 =$ value of x in respect to the point where the dilatation of the bar begins.

$\Xi_2 =$ value of x in respect to the point where the contraction of the bar begins.

* In the case in which there is a resistance of shear of the surfaces as well as of friction; let μ represent the unit of shear corresponding to a unit of surface of 1 foot by 1 inch, and let σ be the area of the surfaces of contact measured in the same units; then $\mu\sigma$ is the resistance of shearing to the descent of the plate. Let also f be the coefficient of friction, then is $fw\sigma\cos\iota$ the resistance of friction. If, therefore, $\tan\phi$ be the coefficient of a friction equivalent to the actual resistance of friction and the resistance to shearing, then

$$w\sigma\tan\phi\cos\iota = \mu\sigma + fw\sigma\cos\iota,$$

$$\therefore \tan\phi = \frac{\mu\sigma\sec\iota}{w} + f. \quad (1')$$

The value of ϕ being determined by this equation, the following discussion includes the case of adherence together with friction, and the resulting formulæ are applicable to that case also.

I.

Case I.—*When the upper end of the plate is fixed.*

Let Δx be a finite increment of x at the temperature T.

When T became $T+t_1$, Δx would become $l_1 \Delta x$ if nothing were opposed to its dilatation. To dilate, it must, however, thrust the portion P B of the plate (fig. 1) down the plane; and the resistance to this displacement is represented by

$$Kw(a-x)f_1.\text{*}$$

Suppose Δx first to have dilated *freely*, so as to have become $l_1 \Delta x$, and then to have sustained a thrust equal to the above-mentioned resistance, and thereby to have been brought back to the dimensions it would have had if it had experienced the resistance from the first.

Let $\delta(l_1 \Delta x)$ represent the compression of the element caused by this thrust. Per foot of the length of the plate this compression is represented by

$$\frac{\delta(l_1 \Delta x)}{l_1 \cdot \Delta x}.$$

But if the same law which holds in respect to small compressions held in respect to all, however great, E lbs. per square inch of section applied as a thrust would, per foot of the length of the plate, produce a compression of one foot; therefore

$$\frac{E\delta(l_1 \Delta x)}{l_1 \cdot \Delta x}$$

will produce a compression of

$$\frac{\delta(l_1 \Delta x)}{l_1 \cdot \Delta x}$$

per foot of length, per square inch of section. But the resistance

$$\frac{Kw(a-x)f_1}{K}$$

produces also this compression;

$$\therefore \frac{E\delta(l_1 \Delta x)}{l_1 \cdot \Delta x} = \frac{Kw(a-x)f_1}{K},$$

$$\delta(l_1 \Delta x) = \frac{wl_1 f_1}{E}(a-x)\Delta x.$$

But

$$\Delta x_1 = l_1 \Delta x - \delta(l_1 \Delta x);$$

$$\therefore\ \Delta x_1 = l_1\Delta x - \frac{wl_1f_1}{E}(a-x)\Delta x,$$

$$\frac{dx_1}{dx} = l_1 - \frac{wl_1f_1}{E}(a-x). \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Similarly, if the temperature T be reduced by t_2, and if the drag upon the element Δx (in the act of contracting) of the portion of the bar below it be applied as a strain after Δx has contracted to $l_2\Delta x$, then this drag being represented, per square inch of section, by $(a-x)wf_2$, we have, as before,

$$\frac{E\delta(l_2\Delta x)}{l_2\Delta x} = w(a-x)f_2,$$

$$\Delta x_2 = l_2\Delta x + \delta(l_2 . \Delta x);$$

$$\therefore\ \Delta x_2 = l_2\Delta x + \frac{wl_2f_2}{E}(a-x)\Delta x,$$

$$\frac{dx_2}{dx} = l_2 + \frac{wl_2f_2}{E}(a-x). \quad . \quad . \quad . \quad . \quad . \quad (2)$$

CASE II.—*When the lower end of the plate is fixed and the rest free.*

Reasoning as before,

$$\frac{E\delta(l_1\Delta x)}{l_1\Delta x} = wf_2(a-x),$$

$$\frac{E\delta(l_2\Delta x)}{l_2\Delta x} = wf_1(a-x),$$

$$\Delta X_1 = l_1\Delta x - \delta(l_1\Delta x),$$

$$\Delta X_2 = l_2\Delta x + \delta(l_2\Delta x),$$

$$\Delta X_1 = l_1\Delta x - \frac{wl_1f_2}{E}(a-x)\Delta x,$$

$$\Delta X_2 = l_2\Delta x + \frac{wl_2f_1}{E}(a-x)\Delta x,$$

$$\frac{dX_1}{dx} = l_1 - \frac{wl_1f_2}{E}(a-x), \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\frac{dX_2}{dx} = l_2 + \frac{wl_2f_1}{E}(a-x). \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

II.

General solution of Cases I. *and* II., *a part of the plate being supposed neither to dilate nor contract.*

Integrating equations (1), (2), (3), and (4), and observing that ξ_1, ξ_2, Ξ_1, Ξ_2 represent the values of x at the points where dilatation and contraction respectively begin,

$$x_1-\xi_1=l_1\left\{\left(1-\frac{wf_1a}{\text{E}}\right)(x-\xi_1)+\frac{wf_1}{2\text{E}}(x^2-\xi_1^2)\right\},$$

$$x_2-\xi_2=l_2\left\{\left(1+\frac{wf_2a}{\text{E}}\right)(x-\xi_2)+\frac{wf_2}{2\text{E}}(x^2-\xi_2^2\right\},$$

$$\text{X}_1-\Xi_1=l_1\left\{\left(1-\frac{wf_2a}{\text{E}}\right)(x-\Xi_1)+\frac{wf^2}{2\text{E}}(x^2-\Xi_1^2)\right\},$$

$$\text{X}_2-\Xi_2=l_2\left\{\left(1+\frac{wf_1a}{\text{E}}\right)(x-\Xi_2)-\frac{wf_1}{2\text{E}}(x^2-\Xi_2^2)\right\}.$$

Reducing,

$$x_1-\xi_1=l_1\left\{1-\frac{wf_1}{2\text{E}}(2a-x-\xi_1)\right\}(x-\xi_1),$$

$$x_2-\xi_2=l_2\left\{1+\frac{wf_2}{2\text{E}}(2a-x-\xi_2)\right\}(x-\xi_2),$$

$$\text{X}_1-\Xi_1=l_1\left\{1-\frac{wf_2}{2\text{E}}(2a-x-\Xi_1)\right\}(x-\Xi_1),$$

$$\text{X}_2-\Xi_2=l_2\left\{1+\frac{wf_1}{2\text{E}}(2a-x-\Xi_2)\right\}(x-\Xi_2).$$

Substituting for l_1, l_2, f_1, f_2 their respective values,

$$x_1-\xi_1=(1+t_1\lambda)\left\{1-\frac{w\sin(\phi-\iota)}{2\text{E}\cos\phi}(2a-x-\xi_1)\right\}(x-\xi_1),$$

$$x_2-\xi_2=(1-t_2\lambda)\left\{1+\frac{w\sin(\phi+\iota)}{2\text{E}\cos\phi}(2a-x-\xi_2)\right\}(x-\xi_2),$$

$$\text{X}_1-\Xi_1=(1+t_1\lambda)\left\{1-\frac{w\sin(\phi+\iota)}{2\text{E}\cos\phi}(2a-x-\Xi_1)\right\}(x-\Xi_1),$$

$$\text{X}_2-\Xi_2=(1-t_2\lambda)\left\{1+\frac{w\sin(\phi-\iota)}{2\text{E}\cos\phi}(2a-x-\Xi_2)\right\}(x-\Xi_2).$$

When x becomes a, x_1, x_2, X_1, X_2 become a_1, a_2, A_1, A_2, respectively. Therefore

$$\left.\begin{aligned}a_1-\xi_1&=(1+t_1\lambda)\left\{1-\frac{w\sin(\phi-\iota)}{2\text{E}\cos\phi}(a-\xi_1)\right\}(a-\xi_1),\\ a_2-\xi_2&=(1-t_2\lambda)\left\{1+\frac{w\sin(\phi+\iota)}{2\text{E}\cos\phi}(a-\xi_2)\right\}(a-\xi_2),\\ \text{A}_1-\Xi_1&=(1+t_1\lambda)\left\{1-\frac{w\sin(\phi+\iota)}{2\text{E}\cos\phi}(a-\Xi_1)\right\}(a-\Xi_1),\\ \text{A}_2-\Xi_2&=(1-t_2\lambda)\left\{1+\frac{w\sin(\phi-\iota)}{2\text{E}\cos\phi}(a-\Xi_2)\right\}(a-\Xi_2).\end{aligned}\right\}(5)$$

Subtracting $(a-\xi_1)$, $(a-\xi_2)$, $(a-\Xi_1)$, $(a-\Xi_2)$ from these equations respectively,

$$\left.\begin{aligned}
a_1-a &= \left\{\lambda t_1 - \frac{w(1+\lambda t_1)\sin(\phi-\iota)}{2E\cos\phi}(a-\xi_1)\right\}(a-\xi_1),\\
a_2-a &= \left\{-\lambda t_2 + \frac{w(1-\lambda t_2)\sin(\phi+\iota)}{2E\cos\phi}(a-\xi_2)\right\}(a-\xi_2),\\
A_1-a &= \left\{\lambda t_1 - \frac{w(1+\lambda t_1)\sin(\phi+\iota)}{2E\cos\phi}(a-\Xi_1)\right\}(a-\Xi_1),\\
A_2-a &= \left\{-\lambda t_2 + \frac{w(1-\lambda t_2)\sin(\phi-\iota)}{2E\cos\phi}(a-\Xi_2)\right\}(a-\Xi_2).
\end{aligned}\right\} \quad (6)$$

Now the values of x represented by ξ_1, ξ_2, Ξ_1, Ξ_2 are those for which $\Delta x = \Delta x_1 = \Delta x_2 = \Delta X_1 = \Delta X_2$. Therefore, by equations (1), (2), (3), (4),

$$1 = l_1 - \frac{wl_1f_1}{E}(a-\xi_1),$$

$$1 = l_2 + \frac{wl_2f_2}{E}(a-\xi_2),$$

$$1 = l_1 - \frac{wl_1f_2}{E}(a-\Xi_1),$$

$$1 = l_2 + \frac{wl_2f_1}{E}(a-\Xi_2).$$

Substituting the values of l_1, l_2, f_1, f_2, and transposing,

$$\left.\begin{aligned}
a-\xi_1 &= \frac{E\lambda t_1\cos\phi}{w(1+\lambda t_1)\sin(\phi-\iota)},\\
a-\xi_2 &= \frac{E\lambda t_2\cos\phi}{w(1-\lambda t_2)\sin(\phi+\iota)},\\
a-\Xi_1 &= \frac{E\lambda t_1\cos\phi}{w(1+\lambda t_1)\sin(\phi+\iota)},\\
a-\Xi_2 &= \frac{E\lambda t_2\cos\phi}{w(1-\lambda t_2)\sin(\phi-\iota)}.
\end{aligned}\right\} \quad . \quad . \quad . \quad (7)$$

Eliminating* $(a-\xi_1)$, $(a-\xi_2)$, $(a-\Xi_1)$, $(a-\Xi_2)$, between equations (6) and (7),

* It is easily seen from this elimination that the dilatation of the whole plate is equal to one half what that of the part of it which dilates would have been if that part had dilated freely. And so of the contraction.

$$\left.\begin{aligned}
a_1 - a &= \frac{E\lambda^2 t_1^2 \cos\phi}{2w(1+\lambda t_1)\sin(\phi-\iota)}, \\
a_2 - a &= -\frac{E\lambda^2 t_2^2 \cos\phi}{2w(1-\lambda t_2)\sin(\phi+\iota)}, \\
A_1 - a &= \frac{E\lambda^2 t_1^2 \cos\phi}{2w(1+\lambda t_1)\sin(\phi+\iota)}, \\
A_2 - a &= -\frac{E\lambda^2 t_2^2 \cos\phi}{2w(1-\lambda t_2)\sin(\phi-\iota)}.
\end{aligned}\right\} \quad . \quad . \quad (8)$$

When the plate is first heated (t_1°) *and then cooled* (t_2°).

Let ${}_1a_2$ be its length after such heating and subsequent cooling when fixed at the top, and ${}_1A_2$ when fixed at the bottom. Then, since a_1 becomes ${}_1a_2$, and A_1 becomes ${}_1A_2$ by a diminution of temperature t_2, we have by the second and fourth of equations (8),

$$ {}_1a_2 - a_1 = -\frac{E\lambda^2 t_2^2 \cos\phi}{2w(1-\lambda t_2)\sin(\phi+\iota)}, $$

$$ {}_1A_2 - A_1 = -\frac{E\lambda^2 t_2^2 \cos\phi}{2w(1-\lambda t_2)\sin(\phi-\iota)}. $$

Adding these equations respectively to the first and third of equations (8),

$$\left.\begin{aligned}
{}_1a_2 - a &= \frac{E\lambda^2 \cos\phi}{2w} \left\{ \frac{t_1^2}{(1+\lambda t_1)\sin(\phi-\iota)} - \frac{t_2^2}{(1-\lambda t_2)\sin(\phi+\iota)} \right\}, \\
{}_1A_2 - a &= \frac{E\lambda^2 \cos\phi}{2w} \left\{ \frac{t_1^2}{(1+\lambda t_1)\sin(\phi+\iota)} - \frac{t_2^2}{(1-\lambda t_2)\sin(\phi-\iota)} \right\}.
\end{aligned}\right\} \quad (9)$$

When it is cooled back to its first temperature, $t_1 = t_2 = t$;

$$\left.\begin{aligned}
\therefore\ {}_1a_2 - a &= \frac{E\lambda^2 t^2 \cos\phi\,(\sin\iota\cos\phi + \lambda t\sin\phi\cos\iota)}{w(1-\lambda^2 t^2)\sin(\phi+\iota)\sin(\phi-\iota)}, \\
{}_1A_2 - a &= -\frac{E\lambda^2 t^2 \cos\phi\,(\sin\iota\cos\phi + \lambda t\sin\phi\cos\iota)}{w(1-\lambda^2 t^2)\sin(\phi+\iota)\sin(\phi-\iota)}.
\end{aligned}\right\} \quad (10)$$

When the plate is fixed at the top, it is *lengthened*, therefore, by being heated and cooled back to the same temperature; and when it is fixed at the bottom, it is as much *shortened*.

III.

General solution of Cases I. *and* II., *every part of the plate being supposed to dilate or contract.*

In this case equations (1), (2), (3), (4) must be integrated between the limits x and 0 instead of between the limits x and ξ_1, ξ_2, Ξ_1, Ξ_2, the results of which integrations may be obtained by making the latter quantities zero in equations (5).

We thus get

$$\left.\begin{aligned}
a_1&=(1+\lambda t_1)\left\{1-\frac{w\sin(\phi-\iota)}{2\mathrm{E}\cos\phi}a\right\}a,\\
a_2&=(1-\lambda t_2)\left\{1+\frac{w\sin(\phi+\iota)}{2\mathrm{E}\cos\phi}a\right\}a,\\
\mathrm{A}_1&=(1+\lambda t_1)\left\{1-\frac{w\sin(\phi+\iota)}{2\mathrm{E}\cos\phi}a\right\}a,\\
\mathrm{A}_2&=(1-\lambda t_2)\left\{1+\frac{w\sin(\phi-\iota)}{2\mathrm{E}\cos\phi}a\right\}a.
\end{aligned}\right\}\quad . \quad . \quad (11)$$

Or, since $\frac{t\lambda}{\mathrm{E}}$ is an exceedingly small quantity,

$$\left.\begin{aligned}
a_1-a&=\quad\left\{t_1\lambda-\frac{w\sin(\phi-\iota)}{2\mathrm{E}\cos\phi}a\right\}a,\\
a_2-a&=-\left\{t_2\lambda-\frac{w\sin(\phi+\iota)}{2\mathrm{E}\cos\phi}a\right\}a,\\
\mathrm{A}_1-a&=\quad\left\{t_1\lambda-\frac{w\sin(\phi+\iota)}{2\mathrm{E}\cos\phi}a\right\}a,\\
\mathrm{A}_2-a&=-\left\{t_2\lambda-\frac{w\sin(\phi-\iota)}{2\mathrm{E}\cos\phi}a\right\}a.
\end{aligned}\right\}\quad . \quad (12)$$

When the plate (every part of which dilates or contracts) is heated t_1° *and then cooled* t_2°.

By the heating the length a becomes a_1 or A_1, according as the fixed point is at the top or the bottom; and by the cooling these lengths become ${}_1a_2$ and ${}_1\mathrm{A}_2$. Therefore by the second and fourth of equations (11),

$${}_1a_2=(1-\lambda t_2)\left\{1+\frac{w\sin(\phi+\iota)}{2\mathrm{E}\cos\phi}a_1\right\}a_1,$$

$${}_1\mathrm{A}_2=(1-\lambda t_2)\left\{1+\frac{w\sin(\phi-\iota)}{2\mathrm{E}\cos\phi}\mathrm{A}_1\right\}\mathrm{A}_1.$$

Eliminating a_1 and A_1 between these equations and the first

and the third of equations (11), and observing that because of the small difference of a_1 from a, and the exceeding smallness of the factors

$$\frac{w \sin(\phi+\iota)}{2\mathrm{E}\cos\phi} \text{ and } \frac{w \sin(\phi-\iota)}{2\mathrm{E}\cos\phi},$$

we may consider

$$\frac{w \sin(\phi+\iota)}{2\mathrm{E}\cos\phi}a_1 = \frac{w \sin(\phi+\iota)}{2\mathrm{E}\cos\phi}a,$$

$$\frac{w \sin(\phi-\iota)}{2\mathrm{E}\cos\phi}\mathrm{A}_1 = \frac{w \sin(\phi-\iota)}{2\mathrm{E}\cos\phi}\mathrm{A},$$

we have

$$_1a_2=(1+\lambda t_1)(1-\lambda t_2)\left\{1+\frac{w \sin(\phi+\iota)}{2\mathrm{E}\cos\phi}a\right\}$$
$$\left\{1-\frac{w \sin(\phi-\iota)}{2\mathrm{E}\cos\phi}a\right\}a,$$

$$_1\mathrm{A}_2=(1+\lambda t_1)(1-\lambda t_2)\left\{1+\frac{w \sin(\phi-\iota)}{2\mathrm{E}\cos\phi}a\right\}$$
$$\left\{1-\frac{w \sin(\phi+\iota)}{2\mathrm{E}\cos\phi}a\right\}a;$$

whence, by reduction,

$$\left.\begin{aligned} {}_1a_2&=a(1+t_1\lambda)(1-t_2\lambda) \\ &\quad\left\{\left(1+\frac{wa\sin\iota}{2\mathrm{E}}\right)^2-\left(\frac{wa\tan\phi\cos\iota}{2\mathrm{E}}\right)^2\right\}, \\ {}_1\mathrm{A}_2&=a(1+t_1\lambda)(1-t_2\lambda) \\ &\quad\left\{\left(1-\frac{wa\sin\iota}{2\mathrm{E}}\right)-\left(\frac{wa\tan\phi\cos\iota}{2\mathrm{E}}\right)^2\right\}, \end{aligned}\right\} \quad . \quad (13)$$

or, approximately,

$$_1a_2=a(1+\lambda t_1)(1-\lambda t_2)\left(1+\frac{wa\sin\iota}{\mathrm{E}}\right),$$

$$_1\mathrm{A}_2=a(1+\lambda t_1)(1-\lambda t_2)\left(1-\frac{wa\sin\iota}{\mathrm{E}}\right).$$

IV.

Case III.—*When no point in the plate is mechanically fixed.*

Since X (fig. 1) is a point so taken that if the plate were cut asunder *there*, the resistance of the part X A to being thrust

upwards would equal that of XB to being thrust downwards when the temperature is raised, an element at X will dilate equally upwards and downwards, and the point X itself (supposed the centre of the element) will remain fixed.

In the same way, since X_1 is a point so taken that the resistance to X_1A being pulled downwards is equal to that to X_1B being pulled upwards if the temperature is lowered, an element at X_1 will contract equally upwards and downwards, and the centre X_1 of that element will remain fixed.

To determine the positions of X and X_1.

Pressure necessary to thrust XA upwards $=Kwf_2\overline{XA}$,

" " " XB downwards $=Kwf_1(a-\overline{XA})$,

" to pull X_1A downwards $=Kwf_1\overline{X_1A}$,

" " " X_1B upwards $=Kwf_2(a-\overline{X_1A})$;

$$\therefore\ Kwf_2\overline{XA}=Kwf_1(a-\overline{XA}),$$

$$Kwf_1\overline{X_1A}=Kwf_2(a-\overline{X_1A}).$$

Whence we obtain, substituting for f_1 and f_2 their values,

$$XA=\tfrac{1}{2}a\frac{\sin(\phi-\iota)}{\sin\phi\cos\iota}=\tfrac{1}{2}a\left\{1-\frac{\tan\iota}{\tan\phi}\right\}. \quad (14)$$

$$XB=\tfrac{1}{2}a\frac{\sin(\phi+\iota)}{\sin\phi\cos\iota}=\tfrac{1}{2}a\left\{1+\frac{\tan\iota}{\tan\phi}\right\}. \quad (15)$$

$$X_1A=\tfrac{1}{2}a\frac{\sin(\phi+\iota)}{\sin\phi\cos\iota}=\tfrac{1}{2}a\left\{1+\frac{\tan\iota}{\tan\phi}\right\}. \quad (16)$$

$$X_1B=\tfrac{1}{2}a\frac{\sin(\phi-\iota)}{\sin\phi\cos\iota}=\tfrac{1}{2}a\left\{1-\frac{\tan\iota}{\tan\phi}\right\}. \quad (17)$$

X and X_1 are therefore symmetrically placed in the bar.

It is evident that while the plate is in the act of *dilatation,* the point X may be considered mechanically *fixed,* and whilst it is in the act of contraction, the point X_1.

The equal and opposite resistances at X and X_1 may, first, equal or be less than the thrust of dilatation, in either of which cases the whole plate will suffer dilatation or contraction; or, secondly, the equal and opposite resistances at X and X_1 may be greater than the thrust of dilatation, in which case a portion only will dilate or contract.

Now the *thrust* with which the plate tends to dilate under an

increase of temperature of $t_1°$ is represented by

$$\frac{\mathrm{KE}t_1\lambda}{1+t_1\lambda}*.$$

And similarly the *strain* with which it tends to contract under a diminution of temperature t_2 is

$$\frac{\mathrm{KE}t_2\lambda}{1-t_2\lambda}\dagger.$$

Whence it follows that the opposite resistances at X and X_1 are respectively greater than the elasticity of the plate, so that a *portion* only dilates and contracts, when

$$\overline{\mathrm{XA}}f_2\mathrm{K}w > \frac{\mathrm{KE}t_1\lambda}{1+\lambda t_1}$$

and

$$\mathrm{X}_1\mathrm{B}f_2\mathrm{K}w > \frac{\mathrm{KE}t_2\lambda}{1-\lambda t_2},$$

or, since

$$\overline{\mathrm{XA}} = \overline{\mathrm{X}_1\mathrm{B}} = \tfrac{1}{2}a\frac{\sin(\phi-\iota)}{\cos\iota\sin\phi},$$

and

$$f_2 = \frac{\sin(\phi+\iota)}{\cos\phi},$$

when

$$\tfrac{1}{2}aw\mathrm{K}\frac{\sin(\phi-\iota)}{\cos\iota\sin\phi}\cdot\frac{\sin(\phi+\iota)}{\cos\phi}\begin{cases} > \dfrac{\mathrm{KE}t_1\lambda}{1+\lambda t_1} \\[2ex] > \dfrac{\mathrm{KE}t_2\lambda}{1-\lambda t_2}, \end{cases}$$

* If no resistance were opposed to the dilatation of the element Δx, it would become, by an increase of temperature t_1, $(1+t_1\lambda)\Delta x$. To bring it back, therefore, to the length from which it has dilated, each foot must be compressed by $\frac{t_1\lambda\Delta x}{(1+t_1\lambda)\Delta x}$. Since, therefore, its section is K, the *thrust* necessary to compress it is represented by $\frac{\mathrm{KE}t_1\lambda\Delta x}{(1+t_1\lambda)\Delta x}$, or by $\frac{\mathrm{KE}t_1\lambda}{1+t_1\lambda}$.

† If R represents the resistance to crushing per square inch of section, and S similarly represents the tensile strength, the bar will crush at X if

$$\frac{\mathrm{KE}t_1\lambda}{1+\lambda t} > \mathrm{KR},$$

and will tear asunder at X_1 if

$$\frac{\mathrm{KE}t_2\lambda}{1-\lambda t_2} > \mathrm{KS}.$$

or generally when

$$a > \frac{1}{w} \frac{\mathrm{E}t\lambda}{(1 \pm t\lambda)} \frac{\sin 2\phi \cos \iota}{\sin (\phi + \iota) \sin (\phi - \iota)}. \quad . \quad . \quad (18)$$

In the case in which this condition is *not* satisfied, or *when the whole plate, having no point mechanically fixed, dilates or contracts by the supposed variation in its temperature*, let $\mathrm{A}_1\,\mathrm{B}_1$ (fig. 2) be what A B becomes when heated by t_1. Then, since X A dilates as it would do if fixed at the bottom, and X B as it would do if fixed at the top, substituting the values (14) and (15) of X A and X B in the third and first of equations (11),

$$\overline{\mathrm{XA}_1} = \tfrac{1}{2} a (1 + \lambda t_1) \frac{\sin (\phi - \iota)}{\sin \phi \cos \iota} \left\{ 1 - \frac{wa \sin (\phi + \iota) \sin (\phi - \iota)}{4\mathrm{E} \cos \phi \sin \phi \cos \iota} \right\},$$

$$\mathrm{XB}_1 = \tfrac{1}{2} a (1 + \lambda t_1) \frac{\sin (\phi + \iota)}{\sin \phi \cos \iota} \left\{ 1 - \frac{wa \sin (\phi + \iota) \sin (\phi - \iota)}{4\mathrm{E} \cos \phi \sin \phi \cos \iota} \right\};$$

$$\therefore\ \mathrm{A}_1\mathrm{B}_1 = a (1 + \lambda t_1) \left\{ 1 - \frac{wa \sin (\phi + \iota) \sin (\phi - \iota)}{2\mathrm{E} \sin 2\phi \cos \iota} \right\}. \quad . \quad (19)$$

Similarly,

$$\mathrm{A}_2\mathrm{B}_2 = a (1 - \lambda t_2) \left\{ 1 + \frac{wa \sin (\phi + \iota) \sin (\phi - \iota)}{2\mathrm{E} \sin 2\phi \cos \iota} \right\}. \quad . \quad (20)$$

If the plate be first subjected to an increase of temperature, becoming $\mathrm{A}_1\,\mathrm{B}_1$, and *then* to a diminution, becoming $\mathrm{A}_2\,\mathrm{B}_2$, the value of $\mathrm{A}_1\,\mathrm{B}_1$ from the former of the above equations must be substituted for a in the latter.

We shall then have approximately,

$$\overline{\mathrm{A}_2\mathrm{B}_2} = a (1 + \lambda t_1)(1 - \lambda t_2) \left\{ 1 - \frac{w^2 a^2 \sin^2 (\phi + \iota) \sin^2 (\phi - \iota)}{4\mathrm{E}^2 \sin^2 2\phi \cos^2 \iota} \right\}. \quad (21)$$

By every such heating and equal cooling the bar will therefore experience an exceedingly small diminution of its entire length.

V.

The descent of the plate when subjected to an increase and then to a decrease of temperature, supposing the whole to dilate and contract.

Observing that X B (fig. 2) dilates as it would do if X were fixed, and substituting for a, in the first of equations (12), the value of XB (equation (15),

$$\overline{\mathrm{BB}_1} = \tfrac{1}{2} a \frac{\sin (\phi + \iota)}{\sin \phi \cos \iota} \left\{ t_1 \lambda - \frac{w \sin (\phi + \iota) \sin (\phi - \iota)}{2\mathrm{E} \sin 2\phi \cos \iota} a \right\}.$$

Observing also that X_1B_1 contracts as it would do if X_1 were fixed, and that in estimating its contraction by substituting X_1B instead of X_1B_1 for a in the second of equations (12), an error will arise only in respect to terms of two dimensions in λ and $\frac{1}{E}$, we obtain as before

$$\overline{B_1B_2}=\tfrac{1}{2}a\frac{\sin(\phi-\iota)}{\sin\phi\cos\iota}\left\{t_2\lambda-\frac{wa\sin(\phi+\iota)\sin(\phi-\iota)}{2E\sin 2\phi\cos\iota}\right\}.$$

Subtracting this equation from the last,

$$BB_2=\frac{a}{2\sin\phi\cos\iota}\left\{\begin{array}{l}\lambda\{t_1\sin(\phi+\iota)-t_2\sin(\phi-\iota)\} \\ -\dfrac{wa\sin(\phi+\iota)\sin(\phi-\iota)\tan\iota}{2E\sin\phi};\end{array}\right\}$$

or, by reduction,

$$BB_2=\frac{a}{2}\frac{\tan\iota}{\tan\phi}\left\{\begin{array}{l}\lambda\left\{(t_1+t_2)+(t_1-t_2)\dfrac{\tan\phi}{\tan\iota}\right\} \\ -\dfrac{wa\sin(\phi+\iota)\sin(\phi-\iota)}{E\sin 2\phi\cos\iota}\end{array}\right\}, \quad . \quad (22)$$

by which equation is determined the descent of the plate after having been heated by t_1 and then cooled by t_2, supposing the whole of it to dilate and contract.

If $t_1=t_2=t$, or if the plate, having been heated by t° above T°, is then cooled down to the temperature T° again,

$$BB_2=a\frac{\tan\iota}{\tan\phi}\left\{\lambda t-\frac{wa\sin(\phi+\iota)\sin(\phi-\iota)}{2E\sin 2\phi\cos\iota}\right.. \quad . \quad (23)$$

The bar descends if

$$t>\frac{wa\sin(\phi+\iota)\sin(\phi-\iota)}{2E\lambda\sin 2\phi\cos\iota}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

VI.

When part only of the plate dilates or contracts by the assumed variation of temperature, no point in it being mechanically fixed, to determine the length.

Substituting the values of XB and X_1B for a in the first and second of equations (8), and XA and X A in the third and fourth,

$$\left.\begin{aligned}
\overline{XB}_1 &= \tfrac{1}{2}a\frac{\sin(\phi+\iota)}{\sin\phi\cos\iota} + \frac{E\lambda^2 t_1^2\cos\phi}{2w(1+\lambda t_1)\sin(\phi-\iota)},\\
X_1B_2 &= \tfrac{1}{2}a\frac{\sin(\phi-\iota)}{\sin\phi\cos\iota} - \frac{E\lambda^2 t_2^2\cos\phi}{2w(1-\lambda t_2)\sin(\phi+\iota)},\\
XA_1 &= \tfrac{1}{2}a\frac{\sin(\phi-\iota)}{\sin\phi\cos\iota} + \frac{E\lambda^2 t_1^2\cos\phi}{2w(1+\lambda t_1)\sin(\phi+\iota)},\\
X_1A_2 &= \tfrac{1}{2}a\frac{\sin(\phi+\iota)}{\sin\phi\cos\iota} - \frac{E\lambda^2 t_2^2\cos\phi}{2w(1-\lambda t_2)\sin(\phi-\iota)}.
\end{aligned}\right\} \quad . \quad (25)$$

Adding the first and third of the above equations and the second and fourth, and reducing,

$$\left.\begin{aligned}
A_1B_1 &= a + \frac{E\lambda^2 t_1^2\sin 2\phi\cos\iota}{2w(1+\lambda t_1)\sin(\phi+\iota)\sin(\phi-\iota)},\\
A_2B_2 &= a - \frac{E\lambda^2 t_2^2\sin 2\phi\cos\iota}{2w(1-\lambda t_2)\sin(\phi+\iota)\sin(\phi-\iota)}.
\end{aligned}\right\} \quad (26)$$

To determine the length of the plate when having been first heated by t_1 *it is cooled by* t_2, *the value* A_1B_1 from the first of the above equations must be substituted for a in the second,

$$\overline{A_2B_2} = a + \frac{E\lambda^2\sin 2\phi\cos\iota}{2w\sin(\phi+\iota)\sin(\phi-\iota)}\left\{\frac{t_1^2}{1+\lambda t_1} - \frac{t_2^2}{1-\lambda t_2}\right\},$$

or

$$A_2B_2 = a + \frac{E\lambda^2(t_1+t_2)(t_1-t_2-\lambda t_1 t_2)\sin 2\phi\cos\iota}{2w(1+\lambda t_1)(1-\lambda t_2)\sin(\phi+\iota)\sin(\phi-\iota)}. \quad . \quad (27)$$

The bar will be lengthened if

$$(t-t_2)>\lambda t_1 t_2, \text{ or if } \left(\frac{1}{t_2}-\frac{1}{t_1}\right)>\lambda.$$

VII.

When the plate is heated ($t_1°$), *to determine what part is not dilated; and when it is cooled* ($t_2°$), *what part is not contracted.*

xX (fig. 2) is the part which, when the plate is heated ($t_1°$), is not dilated; and x_1X_1 is the part which, when the bar is cooled ($t_2°$), is not contracted. In the two cases the points X and X_1 respectively may be considered points mechanically fixed. Therefore taking XB to be represented by a in the first of equations (7), and observing that $a-\xi_1 = XB - Xx = Bx$,

$$Bx = \frac{E\lambda t_1\cos\phi}{w(1+\lambda t_1)\sin(\phi-\iota)}.$$

Similarly, taking xA to be represented by a in the third of equations (7), and observing that $a-\Xi_1 = xA - xX = AX$,

$$\overline{\mathrm{AX}}=\frac{\mathrm{E}\lambda t_1\cos\phi}{w(1+\lambda t_1)\sin(\phi+\iota)}.$$

In like manner,

$$\mathrm{B}x_1=\frac{\mathrm{E}\lambda t_2\cos\phi}{w(1-\lambda t_2)\sin(\phi+\iota)},$$

$$\mathrm{AX}_1=\frac{\mathrm{E}\lambda t_2\cos\phi}{w(1-\lambda t_2)\sin(\phi-\iota)},$$

whence

$$\mathrm{X}x^{*}=a-\frac{\mathrm{E}\lambda t_1\sin 2\phi\cos\iota}{w(1+\lambda t_1)\sin(\phi+\iota)\sin(\phi-\iota)},$$

$$\mathrm{X}_1x_1=a-\frac{\mathrm{E}\lambda t_2\sin 2\phi\cos\iota}{w(1-\lambda t_2)\sin(\phi+\iota)\sin(\phi-\iota)}.$$

If these expressions vanish or become negative, there is no part† of the plate which does not dilate by the assumed increase, and contract by the assumed decrease of temperature.

The fact of the descent of a solid body upon an inclined plane when subjected to alternations of temperature was first observed in the descent of the lead on the southern side of the roof of the choir of Bristol Cathedral, and was communicated to the Royal Society‡ in April 1855. I have since verified it by the following experiment. I fixed a deal board 9 feet long and 5 inches broad to the southern wall of my house so as to form an inclined plane, and upon it I placed a sheet of lead, turning its edges down over the side edges of the board, and taking care that it should not bind upon them, but be free to move with no other obstruction than that which arose from its friction. The inclination of the board was $18^\circ\ 32'$, the thickness of the lead $\frac{1}{8}$ of an inch, its length 9 feet, and its weight 28 lbs. The lower end of the board was brought opposite to a window, and a vernier was constructed which could be read from within, and by which the position of the lead upon the board could be determined to the 100th of an inch. I began to measure the descent of the lead on the 16th of February, 1858, and recorded it every morning between 7 and 8 o'clock, and every evening between 6 and 7 o'clock until the 28th of June.

* If a sheet of lead rest on a plane of oak inclined at $22\frac{1}{2}^\circ$,

$$\mathrm{X}x=a-30{\cdot}63\,t_1,\quad \mathrm{X}_1x_1=a-30{\cdot}63\,t_2,$$

where the length is measured in feet, and the temperature in degrees of Fahrenheit, and the modulus of elasticity of lead is assumed to be 720,000, its coefficient of expansion $\frac{1}{62500}$, and the limiting angle of resistance between it and oak $22\frac{1}{2}^\circ$.

† This agrees with inequality (18).

‡ Proceedings of the Royal Society, vol. vii. p. 341.

The following were the measurements observed during the month of May:—

Date, 1858.	Distance of the lower end of the lead from zero of the vernier, in inches.		Descent in the day.	Descent in the night.	Descent in 24 hours.
	Morning.	Evening.			
May 1.	10·95	11·10	·15		
3.	The lead, overlapping the end of the board by nearly a foot, was this evening drawn back to	0·77			
4.	0·78	1·06	·28	·03	·31
5.	1·09	1·21	·12	·03	·15
6.	1·24	1·44	·20	·10	·30
7.	1·54	1·65	·11	·02	·13
8.	1·67	1·88	·21	·00	·21
9.	1·88	1·93	·05	·07	·12
10.	2·00	2·19	·19	·00	·19
11.	2·19	2·25	·06	·05	·11
12.	2·30	2·33	·03	·03	·06
13.	2·36	2·40	·04	·09	·13
14.	2·49	2·55	·06	·00	·06
15.	2·55	2·68	·13	·06	·19
16.	2·74	2·90	·16	·01	·17
17.	2·91	2·92	·01	·03	·04
18.	2·95	3·08	·13	·08	·21
19.	3·16	3·50	·34	·10	·44
20.	3·60	3·77	·17	·10	·27
21.	3·87	3·87	·00	·03	·03
22.	3·90	4·12	·22	·03	·25
23.	4·15	4·54	·39	·04	·43
24.	4·58	4·64	·06	·00	·06
25.	4·64	5·16	·52	·04	·56
26.	5·20	5·41	·21	·09	·30
27.	5·50	5·84	·34	·01	·35
28.	5·85	6·05	·20	·02	·22
29.	6·07	6·37	·30	·03	·33
30.	6·40	6·55	·15	·08	·23
31.	6·63	6·80	·17		

The *daily* observations were given up on the 31st of May; but the positions of the lead were registered on the 19th, 22nd, 23rd, 24th, and 26th of the following month. The average daily descents in successive months, measured in inches, were—

February.	March.	April.	May.	June.
·1000	·13806	·16133	·21500	·21888

To compare the actual descent on any day with that computed by formula (22), it would be necessary to know, not the extreme temperatures only of the lead on that day, but every oscillation of temperature between those extremes; for every

such oscillation of the temperature up and down in the course of the day and night contributed to the daily descent; and it is the effect of these oscillations, however numerous and however separately small, which that descent totalizes. I accordingly remarked that it was on days when the thermometer in the sun varied its height rapidly and *much* (as on bright days with cold winds, or when clouds were driven over the sun) that the descent was greatest. So remarkably indeed was this the case, that every cloud which shut off the sun for a time from the lead, and every cold gust of wind which blew upon it in the sunshine, seemed to bring it a step down. On the contrary, when the sky was open and clear, and the heat advanced and receded uniformly, the descent was less, although the difference of the extreme temperatures of the day might be greater. It was least of all on days when there was continuous rain. During the night it was often imperceptible—especially in the earlier months of the year, when it was dark from the time of the evening observation to that of the morning. In April and May this interval included a period of sunlight in the early morning, to which the descent registered as having taken place in the night was no doubt due.

XIII. *On the Structure of the Human Ear, and on the Mode in which it administers to the Perception of Sound.* By R. Moon, *M.A., Honorary Fellow of Queen's College, Cambridge**.

I STATED in a former paper† that the human ear is so constructed as to suppress vibrations arising from waves of condensation which become incident upon it, at the same time that it transmits to the sensorium vibrations arising from waves of rarefaction. I now propose to exhibit the grounds upon which I rest this assertion.

The view of the constitution and functions of the organ of hearing which I have just expressed, incredible as it may at first sight appear, will be found, if I mistake not, to dissipate the mystery which has hitherto characterized that most complicated anatomical problem. The circumstances by which this view of the subject was first suggested to me require some words of explanation.

I have elsewhere shown ‡ that if the problem of the propagation of sound through air be pursued by a strict analysis, we shall be led to a conclusion with regard to the velocity of pro-

* Communicated by the Author.

† "On the Theory of Sound." See Phil. Mag. for March last.

‡ See the paper last referred to, and an earlier one, "On the Theory of Pressure in Fluids," in the Phil. Mag. for August 1868.

pagation materially different from that to which a provisional and imperfect theory would conduct us.

I have shown that the velocity with which a small disturbance is propagated through air of a given density is not, as the existing theory would teach us, invariably the same whatever the character of the disturbance,—that, on the contrary, the disturbances capable of such transmission are divisible into two classes, viz. waves of condensation, in which the density is throughout greater, and waves of rarefaction, in which the density is throughout less than the original density of the air through which the propagation takes place*—in waves of the first kind the velocity of propagation being somewhat less, while in waves of the second kind it is somewhat *greater* than the calculated velocity given by the existing theory†.

In arriving at these conclusions I was confronted by this great difficulty, viz. that in a great variety of instances sounding bodies give rise to waves of condensation and waves of rarefaction simultaneously; so that in such instances we should have a double sound whenever the distance of the sounding body from the ear is considerable, unless the ear were so constructed as to suppress one of the two classes of waves.

So incredible did this latter conclusion appear to me, that nothing but the conviction which reiterated examination had wrought in me of the certainty of the results at which I had arrived would have induced me so much as to examine into the evidence upon the subject.

But, however perfect might be the parallelism which I was disposed *à priori* to attribute to waves of condensation and waves of rarefaction as agents for the transmission of sound, the slightest examination of the auditory apparatus was sufficient to show that no such parallelism exists in their modes of action upon the organ of hearing, or in the contrivances by which the latter is adapted to their reception.

The slightest examination was sufficient to show, as I propose by and by to point out, that some of the most striking and characteristic features of the auditory mechanism are specially calculated to transmit the action of rarefied waves, are essential to such transmission, and can exercise no function in the transmission of condensed waves. Nevertheless a long-cherished

* Although waves of condensation and waves of rarefaction are very commonly called into play simultaneously, it may be shown, even upon the principles of the existing theory, that waves of either kind are capable of transmission when no waves of the other kind are present.

† I must be understood to refer here to the theoretical velocity of propagation apart from Laplace's correction, which correction, for the reasons stated in the paper of March last before referred to, I cannot regard as otherwise than untenable.

though erroneous mode of viewing the subject had its natural influence—a false theory leading to false assumptions as to matters of fact—and for a long time prevented my recognition of the truth of which I was in search, and which I now proceed forthwith to establish, viz. *that waves of condensation may be left out of account in considering the phenomena of aëreally transmitted sound.*

The structure of the human ear is described by anatomists with a lucidity and precision than which nothing can be more admirable; but when we turn from the accounts of the structure to the accounts of the functions of the different parts of the organ, all is confused and contradictory*. The subject is undoubtedly beset by great difficulties, two of which have been very generally felt and recognized:—(1) that arising from the supposed double transmission of motion from the tympanal membrane to the labyrinth, viz. through the bones of the ear and by means of the air in the tympanal cavity—in other words, through the fenestra ovalis and through the fenestra rotunda; (2) that due to the fact that very considerable power of hearing, even articulate sounds, often remains after the tympanal membrane has been removed, and the chain of bones hangs loose in, or is absent from the cavity.

Nevertheless I cannot but think that the great difficulty has consisted in the unaccountable and unfortunate propensity† which, so far as I am aware, has characterized every writer on the subject, of considering the effect upon the ear of condensed waves alone—the efforts of each investigator being thus confined to examining the effect of a particular kind of wave upon an organ which, as I hope to show, has been expressly contrived so that waves of that kind shall produce upon it no effect whatever.

* Take, for example, the testimony of Sir John Herschel, delivered so far back as the year 1830, but the justice of which at the present time, I apprehend, few will be inclined to dispute.

"Of all our organs, perhaps the ear is one of the least understood.... In the ear everything is .... obscure. It is not with it as with the eye, where the known properties of light afford a complete elucidation of the whole mechanism of vision, and the use of every part of the visual apparatus."

"In the cavity behind the tympanum is placed a mysterious and complicated apparatus" [the bones of the ear]. See *Ency. Met.* Art. Sound, Nos. 319, 320.

† This propensity is the more surprising when we remember that no one has ever supposed waves of rarefaction to be without their influence in the production of sound, that the least consideration suffices to show that either kind of wave may be propagated without the other, and that in a great number of instances, as for example the sounds produced from a kettledrum, where both kinds of waves occur, rarefied waves head the column.

The human ear may be divided into three principal regions, viz.

(1) The external ear, of which the only portion which here concerns us is the *meatus externus* terminating in the tympanal membrane.

(2) The tympanal cavity, which in the normal state is kept filled with air through the intervention of the Eustachian tube communicating with the throat; which tube is considered to be ordinarily closed, and from time to time opened, during the act of deglutition.

(3) The internal ear or labyrinth, consisting of a chamber or system of mutually communicating chambers enclosed in the solid bone of the skull.

Omitting details unnecessary for our present purpose, the labyrinth may be described as filled with a liquid in which are immersed the nerves through whose agitation the sensation of hearing is produced.

The fluid in the labyrinth is everywhere surrounded by the solid bone, with the following exceptions:—

(*a*) Two small apertures, denominated respectively *fenestra ovalis* and *fenestra rotunda*, where in place of the bone as a boundary are substituted membranes, by which the labyrinth is separated from the tympanal cavity, and by which the liquid in the former is prevented from flowing into the latter.

(*b*) Certain foramina or (so-called) aqueducts, through which the nerves with their attendant blood-vessels which supply the labyrinth communicate with the general nervous and circulating systems.

The sensation of hearing may be occasioned by means of vibrations transmitted through the bone of the skull to the labyrinth; but all articulate sounds, and in general all sounds which are conveyed by the air, are transmitted to the labyrinth through the two fenestræ (ovalis and rotunda) above spoken of.

When the ear is in its normal state (that is, when the tympanum is perfect), all aëreally conveyed sounds become incident on the tympanal membrane in the first instance, and are thence transmitted to one or both of the tympanal fenestræ by a machinery or agency which will be described hereafter. But the agitation of the tympanal membrane is a *sine quâ non* as regards the transmission to the sensitive system of articulate or other aëreally conveyed sounds.

And here it may be observed that if the human tympanum were, as its name implies, a *drum* (that is, a stretched *flat* membrane whose movements are restrained solely by the circular frame upon which it is fixed), no such simultaneous transmission of waves of rarefaction and suppression of waves of condensation as has above been spoken of could possibly take place.

For under such circumstances, if a wave of rarefaction, for instance, were incident upon the tympanum, the pressure of the air without the tympanal membrane being less than the mean pressure, while the air within the tympanal membrane has the mean pressure, a motion of the tympanal membrane—which (if any) would necessarily be a motion outwards—could only take place by reason of the membrane being *stretched*. The occurrence of such a motion outwards would afford decisive proof that the membrane was capable of being stretched; and, being so capable, it would follow, when a wave of condensation was incident upon it (the external being in this case greater than the internal pressure), that motion of the tympanal membrane would again occur, though in this case taking place in a direction contrary to that in which it occurred in the former.

But the tympanal membrane is neither flat, nor are its movements confined simply by the quasi-circular tympanal bone to which it is affixed.

The membrane is concave outwards, convex inwards; from which it results, as will immediately be shown, that the action upon it of rarefied waves and of condensed waves must be radically different.

When a rarefied wave is incident on the membrane, the motion will take place *outwards*; and the membrane being concave outwards, all that is requisite for this is a simple flexure, a simple change of form of the membrane without any stretching, and which may be effected whether the membrane be elastic, or capable of being stretched, or not.

When a condensed wave is incident upon the membrane, on the other hand, the circumstances are altogether different. The motion in this case (if any) must take place inwards; and the membrane, being convex inwards, will be incapable of motion unless it be capable of being stretched. Nor would a mere capacity for being stretched be sufficient to allow of continuous action of the ear for auditory purposes. The membrane must possess the power of speedily returning to its original status; *i. e.* it must be highly elastic.

When the ear is in its normal state, therefore, it clearly appears that, in order to the transmission to the sensorium of the vibrations of a rarefied wave, flexibility of the tympanal membrane without elasticity is sufficient; while for the like transmission of the vibrations of condensed waves elasticity of the membrane is essential.

What, then, is the character as regards elasticity of the tympanal membrane? The membrane is thus described by the late Mr. Toynbee:—

"Looked at from without inwards, the membrana tympani

may be described as consisting of the following layers:—(1) the epidermis; (2) the dermis; (3) the fibrous layer, composed of (*a*) the lamina of radiating fibres, (*b*) the lamina of circular fibres; (4) the mucous membrane*.

It thus appears that the tympanum is a compound membrane consisting of five layers which are mutually adherent, two of the layers partaking of the character of *fibrous membrane.*

Dr. Brennan† has furnished a Table of the principal organic tissues in the order of their elasticity, which I give complete as follows:—

(1) Yellow fibrous tissue, (2) cartilage, (3) fibro-cartilage, (4) skin, (5) cellular membrane, (6) muscle, (7) bone, (8) mucous membrane, (9) serous membrane, (10) nervous matter, (11) fibrous membrane.

It thus appears that the tympanal membrane, instead of being highly elastic, as it has been shown that it ought to be in order to admit of the motion produced by waves of condensation being transmitted through the tympanum, involves in its composition, and has its elasticity measured by that of fibrous membrane, which is the least elastic and the most unyielding of all the organic tissues, as to which Dr. Brennan observes that it "is remarkable for its low degree of elasticity." And that we may be certain that the particular membrane of the tympanum is no exception to the rule with regard to fibrous membrane in general, we have the following testimony of Mr. Toynbee:—

"Neither do the component fibres of the laminæ appear to evince more than an extremely slight degree of elasticity." (Diseases of the Ear, p. 134‡.)

Other arguments in favour of the position which I have been seeking to establish will hereafter be adduced; and in particular I shall endeavour to show that the auditory apparatus deprived of the tympanal membrane, equally with the apparatus in its normal state, is calculated to transmit waves of rarefaction and to suppress waves of condensation; but in the mean time I would ask whether, if it had been the design of nature to secure such transmission and suppression respectively in the perfect ear, any construction of the tympanal membrane could have been devised better calculated to accomplish those objects than that which actually occurs—the concavity of the membrane combined with its flexibility ensuring the transmission of rarefied waves, whilst the same concavity combined with inelasticity forbids the transmission of condensed waves.

* Diseases of the Ear, with Supplement, by Hinton. London, 1868.

† Todd's 'Cyclopædia of Anatomy and Physiology,' vol. ii. p. 60.

‡ "On n'y trouve point de fibres élastiques."—*Traité d'Anatomie descriptive*, par Cruveilhier. Paris, 1868, vol. ii. p. 674.

The argument is not limited, however, to a bare demonstration that the ear is open to the action of one class of waves while it suppresses the action of the other. It may be shown that some of the most remarkable and characteristic portions of the auditory apparatus are expressly contrived with a view to facilitate and regulate the admission and transmission of waves of rarefaction, and have no intelligible function as applying to the transmission of condensed waves.

If the tympanal membrane were capable of being stretched when a condensed wave becomes incident upon it, it is quite certain that its elasticity, *i. e.* its tendency to recover its original form, would be sufficient to bring it back to its original position and *status*.

But when, through the incidence upon it of a rarefaction, the membrana tympani is pushed outwards, what is to bring it back to its original position? There is no property of the membrane itself capable of producing this effect. A distinct machinery is requisite for the purpose; and this machinery we have in the muscles acting upon the bones of the ear.

To make this clear, it will be necessary to view more in detail the structure of the organ.

The tympanal membrane is connected with the fenestra ovalis by a chain of small bones, variously estimated as three and four in number, but which for our present purpose may be regarded with sufficient accuracy as consisting of three, stretching across the tympanal cavity, and respectively denominated:—(1) the malleus, next to the membrana tympani; (2) the incus; (3) the stapes, or stirrup bone, whose name describes its shape, the base of which is attached to the membrane of the fenestra ovalis.

The three bones or ossicles are articulated upon one another in the order in which they have been named. The body of the malleus and the body of the incus, which are in juxtaposition, are much more massive than the other portions of the ossicular system. The former puts out a comparatively slender arm called the handle of the malleus, which extends from the side of the tympanal cavity to about the centre of the membrana tympani. At the centre of the membrane, and nearly along the entire length of the handle of the malleus, the latter is attached to the membrane and moves with its motion.

The incus sends out a slender process on the other side to the apex of the stirrup, to which it is attached.

The base of the stapes is described by Sir W. Wilde as fitting into the fenestra ovalis "somewhat like a stopper or the piston of a cylinder, and is attached to its circumference by a ligamento-fibrous membrane."

When the membrana tympani moves outwards, as it will do when a rarefied wave is incident upon it, it carries along with it the handle of the malleus, and the tendency will be to pull out the base of the stirrup-bone, a tendency which, no doubt, will be in some degree yielded to*. And we may thus see how the incidence of a rarefied wave may give rise to motion of the fluid in the labyrinth, and consequently to such an excitation of the auditory nerve as will occasion the perception of sound.

It has been already observed that when the membrana tympani has moved outwards, it has no property by which it can restore itself to its original position.

This function is performed by another and most important part of the auditory apparatus—to wit, the muscles of the ear, which are thus described by Mr. Wharton Jones.

"Some anatomists admit four muscles—three attached to the malleus, and one to the stapes. Of the three attached to the malleus, two are described as having for their action the relaxation of the membrana tympani; but these so-called laxatores tympani are merely ligaments..... Two muscles only can be strictly demonstrated, and these two are both tensors of the tympanum." (Cyclop. Anat. and Physiology, vol. ii. p. 547.)

Of these two muscles, the principal (tensor tympani) is attached to the anterior surface of the handle of the malleus; and by its action "*the handle* of the malleus is drawn inwards and *forwards*, whilst the head is moved in the opposite direction... The result of this movement of the bone is that the membrana tympani....is also drawn inwards and stretched." In addition to which, "the base of the stapes is forced against the vestibular fenestra, in consequence of the movement communicated by the head of the malleus to the incus, which tends to press inwards the long extremity of the latter." (Ibid. p. 549.)

The second and smaller of the muscles (stapedius) is "inserted into the posterior and upper part of the head of the stapes."

"The first effect of the action of this muscle will be to press the *posterior* part of the base of the stapes against the vestibular fenestra. At the same time the long branch of the incus will be drawn backwards and inwards, and *the head* of the malleus being by this movement of the incus pressed *forwards* and outwards its handle will be carried inwards, and the membrana tympani thus put upon the stretch." (Ibid. p. 549.)

It thus appears that it is the effect of both muscles:—

* The action which takes place along the chain of bones is exactly that which occurs along the bell-wires when a chamber-bell is rung. Of the degree in which the stapes will yield to the tendency to pull it out more will be said hereafter.

(1) To draw backwards and stretch the membrana tympani;

(2) To force inwards the stapes*;

that is, the effect of the muscles combined with the bones of the ear is to produce in the stapes and membrana tympani a motion opposite to that produced in them by rarefied waves of air.

Hence, since in order that the auditory apparatus shall continue in the exercise of its proper functions it is essential that it shall possess in itself the means of restoration to its normal state after disturbance—since, as has been seen, the combined bones and muscles of the ear are adequate to perform this function as regards rarefied waves—since no other mode of performing it is apparent—and since no other intelligible function has ever been ascribed to this combination of bones and muscles†, we are justified in concluding that that most remarkable and characteristic portion of the auditory mechanism (the muscles of the ear) has been provided solely with reference to the action upon the organ of rarefied waves.

It has been already stated that when the membrana tympani moves outwards, its tendency to pull out the stapes will be in some degree yielded to. The whole scheme of the contrivance

* The late Mr. Toynbee (Diseases of the Ear, p. 177) appears to have entertained the opinion that the two muscles have opposite functions.

I think we may conclude with certainty that such cannot be the case; for otherwise, the muscles being of the voluntary class (Wilde's 'Practical Observations on Aural Surgery,' 1853, p. 314), a person in the midst of the most absolute silence might by a mere exercise of volition produce all the effects occasioned by actual sounds.

When the stapes is drawn home (that is, is forced as far as possible into the vestibule), there can be no doubt that if the tendon of the stapedius were pulled, the effect would be slightly to pull out the stapes, and at the same time slightly to relax the membrana tympani. But, apart from the question as to how far the muscle would act when the bone was in this position, it is evident that if instead of being driven inwards the stapes had been forced outwards, as it would be by the action of rarefied waves, any action of the stapedius muscle consequent thereupon would be to draw the stapes inwards and to stretch the membrana tympani.

A careful consideration of the passages above cited from Mr. Wharton Jones will show that when the tensor tympani is exercised, the effect, amongst other things, is to produce a pressure on the anterior extremity of the vestibular fenestra and a slight rotation upon it, to counteract which is, in the perfect ear, the special function of the muscle of the stapes.

† Mr. Toynbee considered, and others have concurred with him in this opinion, "that the function of the tensor tympani muscle is to protect the membrana tympani and the labyrinth from injury by loud sounds." (Diseases of the Ear, p. 179.) Since the action of the tensor tympani takes place in the same direction as the action (if any) of condensed waves, it is not easy to see how the tensor tympani could *diminish* the effect of the latter on the membrana tympani and labyrinth. On the other hand, since the action of rarefied waves on the tympanum is opposite to that of the tensor tympani, we can comprehend how, when rarefied waves are incident, the tensor tympani might operate to mitigate their effect.

requires that such should be the case; but the mode in which this effect occurs demands very careful consideration.

If there were no round aperture, it is clear, either that such effect could not occur at all, or could occur only to an extent almost, if not absolutely imperceptible, and certainly very much less than the structure of the stapes with the membrane attached to it is calculated to admit of. For in such case the labyrinth would be a closed vessel filled with liquid, and in all parts rigid except at the oval aperture. Consequently the vacuum which the motion outward of the stapes would tend to produce must be filled up by the liquid contents of the labyrinth, a result which could only occur (1) through an expansion of the liquid in the labyrinth, or (2) through a contraction in the space occupied by that liquid by reason of the expansion of the walls and solid contents of the labyrinth. It may well be doubted whether the expansion of the liquid in the labyrinth, or the contraction in the space occupied by that liquid through the agency just referred to, would be traceable by the aid of the finest instruments, whereas the extent to which the stapes may vibrate is perceptible, I apprehend, to the naked eye. We may conclude, therefore, that the existence of the fenestra rotunda is essential to the production in the stapes of that degree of motion of which it is susceptible.

The mode in which the fenestra rotunda operates for that purpose may be gathered from the following passage from Sir W. Wilde.

"That the membrane [of the fenestra rotunda] vibrates is proved by experiment; and one use of it may be to allow the fluid contained within the vestibule, when pressed upon by the base of the stapes (covering like a lid the fenestra ovalis), to bulge a little into the cavity of the tympanum." (Practical Obvations &c., p. 312*.)

Assuming that such is the case when the organ is in its normal state, the membrana tympani being drawn inwards†, the stapes

* I take the following still more decisive testimony from one of an interesting series of papers in the 'Lancet' by Dr. Allen. "The tensor tympani influences principally and chiefly the drumhead by pulling inwards the handle of the malleus and the membrane in which it is imbedded; and in the second, but not less important, place, it stretches the membrane of the round cochlear opening by pressing the base of the stapes into the oval vestibular opening, and driving the liquor Cotunnii (or labyrinth fluid) through the scalæ against the inner surface of the membrane [of the round aperture] and causing it to bulge outwards." (See 'Lancet' for May 1, 1869.)

† According to Politzer (cited by Mr. Hinton), the act of swallowing will produce this effect by diminishing the pressure of the air in the tympanal cavity. (Diseases of the Ear, p. 443.)

pressed home, and the membrane of the fenestra rotunda bulging out into the tympanal cavity, it is evident that when a rarefied wave becomes incident upon the membrana tympani, the latter will move outwards, drawing the stapes from the labyrinth, the fluid in the latter following the stapes by reason of the pressure of the air in the cavity of the tympanum on the membrane of the cochlear fenestra, which would thus be driven inwards.

I think that the foregoing remarks will have made evident what are the true relative functions of the two apertures from the tympanal cavity into the labyrinth. So long as disturbance was supposed to be transmitted along the chain of bones exactly in the same manner as if they had constituted a rigid bar, without producing in any degree that opening or shutting of the labyrinth which the whole scheme of the mechanism proves is possible, and was intended to be produced, the supposition that a like transmission took place through the air in the tympanal cavity was a perfectly natural and proper one. But if it be admitted that the stapes is so fitted to the vestibular aperture as to admit of being pushed inwards and outwards—if the action of a rarefied wave on the membrana tympani is to pull it outwards, while the action of the muscles of the ear is to pull it inwards—and if, as we have seen, none of these capacities or tendencies can be carried into effect unless the action of the cochlear membrane be such as we have described it, it is clear that the action of the two fenestræ must be opposite to each other—the one tending to move in as the other tends to move out, and *vice versâ*, the two thus combining to produce that one effect (to wit, the agitation of the fluid in the labyrinth) which is essential to the perception of sound.

But although I consider the explanation above offered sufficient, so far as relates to the action of the perfect ear, it is evident that when the membrana tympani is destroyed, or, being perfect, the ossicular connexion between it and the labyrinth is broken, the above reasoning ceases to be applicable; and yet in these latter cases a very considerable amount of auditory power is frequently retained.

I think, from what has preceded, we are entitled to assume that it is the function of the muscles of the ear to restore the auditory apparatus to its normal position of equilibrium*; whence it will follow, even where the membrana tympani is

* In confirmation of this view, I cite the following passages from Cruveilhier.

"La base de l'étrier, est une plaque mince... dont la configuration est exactement adaptée à celle de la fenêtre ovale, qu'elle remplit parfaitement, *et dont on ne la retire qu'avec un léger effort ; en sorte que*

absent, or its connexion with the labyrinth is destroyed, that the membrane of the round aperture, when in its normal position, will bulge out into the tympanic cavity,—such bulging out resulting, in the first of the cases now spoken of, it may be, from the united action of both the muscles of the tympanum, while in the latter it must be due to the operation of the stapedius alone.

Such being the case, a condensed wave which became incident upon the ear under such circumstances would be stopped by the membrana tympani, if that membrane were perfect; or if it were absent, the condensed air pressing upon the stapes could have no operation to force it further into the labyrinth, that bone, through the operation of the stapedius muscle, being supposed to have been already driven as far into the labyrinth as the shape of the aperture, or the liquid in the labyrinth, would allow.

The manner of the suppression of condensed waves, when the tympanal membrane is destroyed, thus readily appears. The mode of operation of rarefied waves under similar circumstances, or when, the membrana tympani being present, there is disconnexion in the chain of bones, is a matter of greater delicacy.

In this case, to produce that combined action of the stapes and cochlear membrane which in the perfect ear has been shown to be essential in order to occasion the perception of sound, we must have, when a rarefied wave is incident, a variation in the external pressure on the two fenestræ. Such a variation of pressure I conceive would necessarily arise from the different positions which the two apertures into the labyrinth occupy with respect to the meatus externus, the base of the stapes being nearly centrically opposite, and in a plane parallel to the position which would be occupied by the tympanal membrane if the latter were present*, while the cochlear membrane

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l'étrier a plus de tendance à tomber dans le vestibule que dans la caisse du tympan " (vol. ii. p. 680).

"La paroi externe de la cavité du vestibule . . . présente l'orifice de la fenêtre ovale; mais cet orifice est si parfaitement comblé par la base de l'étrier, que cette circonstance ne trouble l'aspect lisse et égal de cette paroi" (vol. ii. p. 691). See also Henle's *Handbuch &c.* vol. ii. p. 758.

* For the foregoing statement I rely on the general tenor of the accounts I have read upon the subject, and on observation of preparations of the part in the dry bone which I have had an opportunity of examining in the Museum of the Royal College of Surgeons in London. As confirmatory, so far as they go, I would refer to the plates in Cruveilhier, vol. ii. pp. 669 and 693 (given also in Dr. Henle's *Handbuch der systematischen Anatomie des Menschen*, vol. ii. pp. 731, 760), and to that in Dr. Allen's paper in the 'Lancet' for January 16, 1869.

is oblique to the latter—the vestibular aperture being opposed directly to the full stream of the wave, while the cochlear aperture is exposed to it obliquely, and, as I apprehend, though I speak less confidently as to this point, laterally with respect to the main stream of the incident wave*.

The difference of pressure thus occurring at the opposite extremities of the labyrinth will necessarily cause a motion of the stapes outwards, to counteract which the muscle of the stapes will be called into play, so as to produce eventually a motion in the opposite direction—the same action in the labyrinth being thus occasioned which it has already been shown occurs when the ear is in its normal state, and which, I would submit, the whole scheme of the apparatus shows to be essential in order to cause in the human subject the sensation of hearing.

The question here naturally arises—If, the tympanal membrane being absent and the malleus, incus, and Eustachian tube being deprived of all intelligible function, the ear is so competent an instrument for the perception of sound, what can have led to the adoption of the complicated apparatus, the items of which have just been enumerated?

The consideration of this question, as of other points of the greatest interest connected with the subject, I must reserve to some future occasion.

6 New Square, Lincoln's Inn.
June 22, 1869.

XIV. *On the Mechanical Principles involved in the Sailing Flight of the Albatros. By* Captain F. W. Hutton, *F.G.S.*†

UNTIL lately no subject in ornithology had been less successfully treated than that of flight, notwithstanding its great interest. This, no doubt, is owing to the great difficulty of the problem; for not only has the mechanism of the organs of flight to be perfectly understood, but the complicated question of the resistance of the air to differently shaped surfaces moving with variable velocities must also be more or less completely solved. The first part (*i. e.* the mechanism of the organs of

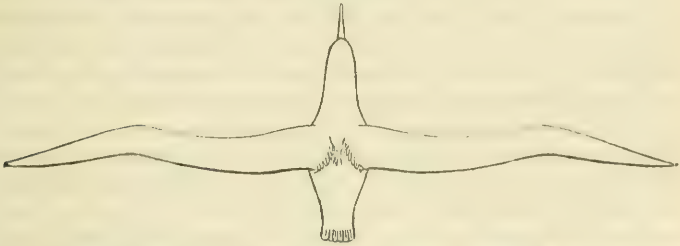
* The assumption that the obliquity of the cochlear fenestra will affect the pressure upon its membrane implies, of course, a variation of pressure in the incident wave according to the direction in which it is estimated. In the March paper above referred to I have shown that when a pulse is propagated along a tube, the vibration being parallel to the axis, a diminution in the pressure exerted on a plane perpendicular to the axis will be due to the velocity. I see no reason to suppose that under the same circumstances any change will occur in the pressure on a plane parallel to the axis.

† Communicated by Alfred Newton, M.A., F.L.S. &c.

flight) has recently been very ably and fully discussed by the Duke of Argyll in 'The Reign of Law,' and by Dr. Pettigrew in the Transactions of the Linnean Society, vol. xxvi.; the second, however, as far as I know, has never been attempted; and I propose therefore to make a few remarks on the "sailing" flight of the Albatros (*Diomedea exulans*, L.), and try to determine approximately the probable resistance of the air in order to allow it to sail for half an hour without moving its wings. Before commencing, however, it may be necessary to remark that the velocities spoken of are velocities of the bird through the air, and not over the water; for the latter will be very different when a wind is blowing.

I estimate the under surface of the wings, body, and tail of the Albatros to be about 8 square feet (see fig. 1); and if we take

Fig. 1.



the weight of the bird to be 16 lbs., we find that it would take a pressure of 2 lbs. per square foot to support it in the air. This pressure would be given by an upward current of air having a velocity of 31 feet a second if the surface acted upon were flat: but the wings of the bird when sailing are bent downwards (see fig. 2), which would increase the resistance; on the other hand,

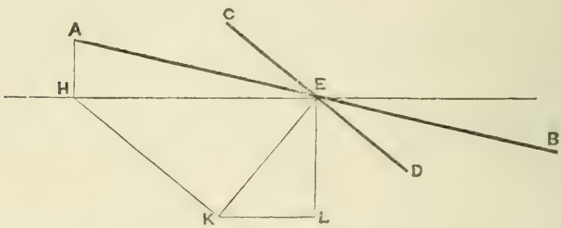
Fig. 2.

the body of the bird is convex, and the wings are inclined at an angle to the horizon, both of which would decrease the resistance, while the surface of the wings is about three times as large as the surface of the body and tail. Balancing one against the other, we perhaps shall not underestimate it if we take an upward current of air with a velocity of 30 feet per second as sufficient to support it. This, in other words, means that on a perfectly still day an Albatros with its wings outstretched, but with no forward movement, would fall downwards at a constantly increasing rate until it had attained a velocity of 30 feet per second,

which velocity it would maintain until it fell into the sea. This is called its "terminal velocity."

Let A B represent the axis of the body of the bird flying in

Fig. 3.



the direction B A and at an angle A E H with the horizon. Let C D represent the wings of the bird making an angle C E H with the horizon. Take the line H E to represent the velocity at which the bird is flying, or the number of feet it passes through the air in one second. From H draw the perpendicular H A; this line will represent the distance which the bird will rise (omitting for the present the force of gravity) by means of the angle at which he is flying to the horizon. But the force of the wind H E acting upon the inclined wings C D will be resolved into two forces, one of which, H K, will be parallel to the wings and so have no effect on them, while the other, K E, will be at right angles to them. This force will be again resolved into two others at right angles to one another—one, K L, opposing the forward movement of the bird, and the other, L E, causing it to rise; so that the total amount that the bird will rise per second will be LE + HA feet. But we have previously seen that it will fall by the action of gravity 30 feet a second; so that in order that it may fly horizontally, without either rising or falling, LE + HA must equal 30; and we want to find what must be the length of HE, or, in other words, the velocity of the bird to do this.

Now $KE = HE \sin CEH$, because CEH is equal to EHK, and LE is equal to $KE \cos CEH$, because KEL is also equal to CEH. Therefore

$$LE = HE \sin CEH \cos CEH, \text{ and } AH \text{ equals } HE \tan AEH;$$

$$\therefore\ HE \tan AEH + HE \sin CEH \cos CEH = 30,$$

$$HE\,(\tan AEH + \sin CEH \cos CEH) = 30;$$

$$\therefore\ HE = \frac{30}{\tan AEH + \sin CEH \,.\, \cos CEH}.$$

If, now, we take $AEH = 0$ and $CEH = 15°$, we shall find that HE equals 115. If we take $AEH = 7°$ and $CEH = 22°$, we find

that HE equals 64. So that if an Albatros starts with a velocity of 115 feet a second, it could maintain a constant height above the sea until its velocity was reduced to 64 feet a second by merely increasing the angle to the horizon at which it was flying from 0° to 7°.

The velocity of the air in a "fresh sailing-breeze" is about 30 feet a second, in a "moderate gale" 60 feet a second, in a "strong gale" 90 feet a second, and in a "great storm" 120 feet a second. Now an Albatros can often be seen sailing, though slowly, directly against a strong gale; his velocity must therefore often be more than 90 feet a second; he is, however, most at home in a strong breeze or moderate gale, when the velocity of the wind is 50 or 60 feet a second, and consequently when his velocity would have to be 70 or 80 feet a second to enable him to fly easily against it. In a calm or light air, when the wind has a velocity of only 10 feet a second, the Albatros rarely sails for so long as a minute at a time—the reason for this being that as, in order to sustain himself in the air, he must move through it with a velocity not less than 64 feet a second, he would, even when flying against the wind, have to travel over the sea at the rate of not less than 54 feet per second, or 36 miles an hour, and so could not reach it properly for good, nor stop himself quick enough when he saw anything; so that the velocity and manner of flight observed in the Albatros correspond closely enough with those calculated as necessary from theoretical considerations.

We will now proceed to see what the resistance of the air to his forward progress ought to be to enable him to start with a velocity of 115 feet a second and sail for half an hour without flapping his wings, and at the end of that time to have reduced his velocity to 64 feet per second.

If a body starts with a velocity V, and after moving for t seconds the resistance of the air reduces its velocity to v, it can be shown that

$$\frac{1}{v}-\frac{1}{\mathrm{V}}=\frac{\mathrm{A}gkt}{\mathrm{W}}, \quad \ldots \ldots \quad (1)$$

where W represents the weight of the bird in pounds, A the area of its front surface in square feet, g the force of gravity, and k a constant quantity depending on the form of the surface exposed to the air, and probably on the velocity at which the body moves; so that, in order to find this constant for the Albatros, we have

$$k=\left(\frac{1}{v}-\frac{1}{\mathrm{V}}\right)\frac{\mathrm{W}}{gt\mathrm{A}}$$

$$=\frac{\mathrm{W}\,.\,(\mathrm{V}-v)}{\mathrm{V}\,.\,v\,.\,g\,.\,t\,.\,\mathrm{A}}.$$

If we take A, in the case of the Albatros, to represent one square foot, and put the other values into the equation, we get

$$k=\frac{16\times 51}{115\times 64\times 32\times 1800\times 1},$$
$$=0\cdot 000002;$$

so that the formula for the resistance of the air to the Albatros ought to be

$$R=0\cdot 000002\,v^2.$$

The formula given by Poncelet for the resistance to round shot is

$$R=0\cdot 0006\,Av^2.$$

If, therefore, these calculations are tolerably correct, the resistance offered to the Albatros must be $\frac{1}{300}$ of that offered to round shot. This at first sight seems to be impossible; but I must remark, *first*, that the terminal velocity of the bird may be less, and the angle at which it flies to the horizon greater than those that I have taken, either or both of which would reduce the velocity at which it was compelled to sail in order to support itself in the air; *secondly*, that the resistance of the air to the flight of elongated projectiles seems to be very much less than that to round shot; but I have seen no experiments on the subject; and as the shape of the Albatros is perhaps the best that could be devised for penetrating the air (see fig. 1), the resistance it had to overcome would undoubtedly be considerably less than that offered to the best-shaped projectile; and *thirdly*, that the formula, as obtained by experiment, for round shot does not pretend to absolute correctness, and applies to projectiles starting with an initial velocity of 1200 feet a second; and it is highly probable that the law that the resistance decreases as the square of the velocity does not hold good for small velocities such as those we are now considering. For example, the range of the larger mortar-shells, which start with an initial velocity of 300 to 400 feet per second, is much more truly calculated by the parabolic theory, which omits the resistance of the air altogether, than by allowing for it by means of the formula $R=0\cdot 0006\,Av^2$. Still the resistance to the Albatros seems very small, and it would be interesting to try to obtain it experimentally. From formula (1) we obtain

$$t=\frac{W(V-v)}{V\,.\,v\,.\,g\,.\,k\,.\,A},$$

by which we see that weight is necessary for a bird to be able to *sail*, and that the greater the weight the longer it can continue to sail; but I cannot agree with the Duke of Argyll (Reign of Law,

p. 152) and Dr. Pettigrew (Trans. Linn. Soc. vol. xxvi. p. 218) that weight is absolutely essential for ordinary *flight*. The fact of many birds diving and catching fish under water is a sufficient refutation of this view, as diving is only flying in water, or in a medium of greater specific gravity than the body of the bird; for all birds, even the Penguin, are lighter than water and float upon it when shot; but, as Dr. Pettigrew has said (p. 214), the wings must in this case act differently, as they have to overcome an upward force of gravity instead of a downward one.

As the resistance of the air decreases as the square of the velocity, it is evident that low velocities are favourable for long-continued sailing, although practically these velocities must be regulated by the velocity of the wind that is necessary to sail against. Now low forward velocities depend upon the bird having a small terminal velocity, which in its turn depends to a great extent upon a large under surface for the air to act upon, so that it may be said that the sailing-powers of a bird depend upon its weight and the expanse of its wing in proportion to its weight,—weight enabling, indeed compelling, it to fly, and expanse of wing enabling it to sail for a long time. For these reasons I cannot agree with the Duke of Argyll (p. 157 *et seq.*) and Dr. Pettigrew (pp. 216 & 257) that long narrow wings are essential for sailing, and I appeal to the Condor, the Vulture, and the Great Bustard to bear me out. In India I have often lain on my back and watched through a telescope the vultures sailing high up in the sky, and have never seen the slightest movement of a wing; and in the Crimea, on the plains of the Alma, I have been astonished at the sailing-powers possessed by the Great Bustard (*Otis tarda*), having once seen it wheeling round in large circles for several minutes without moving its wings. Long and pointed wings, however, are necessary for turning quickly; and the Albatros could not top the waves so neatly as he does if his wings were shaped like those of the Condor, which, soaring high in the air, has no necessity for sharp turns, and consequently for sharp-pointed wings. I may here remark that it is quite easy to understand, on these principles, that a bird having a very large expanse of wing in proportion to its weight, might sail for a very long time on a calm, or nearly calm day, when there was no wind to carry it away, and when consequently its velocity might be very slow. If, now, for the sake of comparison, we take the Cape-pigeon (*Procellaria capensis*) and assume the area of its under surface to be 2·5 square feet, and the area of its front surface to be 0·25 square foot, its weight being, from my own observations, 14 oz. or 0·88 lb., we find that it would have a terminal velocity of 13 feet per second, which, when flying at the same angles as we have taken for the Albatros, will

give velocities of 52 and 29 feet per second respectively. Therefore

$$t=\frac{0{\cdot}88\times 23}{52\times 29\times 32\times 0{\cdot}000002\times 0{\cdot}25},$$

$$t=843 \text{ seconds or } 14 \text{ minutes;}$$

so that the Cape-pigeon could sail half as long as the Albatros, the resistance of the air being supposed to be proportionately the same in both cases. This is more than we should expect, considering the great difference of weight of the birds, but is owing to the small terminal velocity of the Cape-pigeon. It must, however, be observed that although it seems that under favourable circumstances a Cape-pigeon could sail for 14 minutes, the velocity of 29 feet a second is so small that, in order to make headway against the wind, it would have to stop sailing and use its wings long before it had reached its least possible velocity; so that it could not sail for long without being carried away by the wind, neither could it sail at all in a strong gale, except when sheltered by the waves; and this answers very well to what we observe; for in a gentle air the Cape-pigeon sails longer than the Albatros, but hardly ever in a gale. Once during a fresh gale, the air moving probably 70 or 80 feet a second, when standing at the stern of the ship, a Cape-pigeon was blown into my hands and I caught it.

In the foregoing brief remarks I do not pretend to have done more than indicate the principles involved in the flight of the Albatros when sailing along without moving its wings. The problem still remains to be solved; but until some experiments have been made on the resistance offered to the air by the front and lower surfaces of birds, a tolerably accurate solution is not possible; and I hope that some person with the necessary opportunities and means may be induced to take up this highly interesting subject.

XV. *Note on a new Fluorescent Substance.* *By* John Parnell, *M.A., F.R.A.S.**

When aniline is heated with mercuric chloride, besides the ordinary formation of aniline-red, a substance is produced in no inconsiderable quantities which possesses such a remarkable fluorescence that the author, not having been able to find any notice of it hitherto published, cannot but think it must up to the present time have escaped observation. The

* Communicated by the Author.

crude mass obtained by the process above mentioned, when pounded, mixed with water, and washed with ether, gives an ethereal solution which in a concentrated state exhibits a fluorescence which it is believed has never been surpassed by any known body*. By this means, however, the powdered mass is apt to cake together, so that it is difficult to extract all the substance in question, which, to avoid periphrasis, it is proposed temporarily to call Fluoraniline. A better method appears to be to dissolve the crude mass in dilute hydrochloric acid, to add ammonia in excess, and then to wash out with ether. The ethereal solution thus obtained must be repeatedly washed with water until the washings cease to acquire a pink colour. Thus purified it has a greenish-yellow colour and exhibits a green fluorescence. When evaporated to dryness spontaneously, the residue consists of two amorphous substances, one red and the other orange, the fluorescence being due apparently to the latter. The author has not succeeded at present in perfectly eliminating the red substance, although it may be got rid of to a great extent by washing the ethereal solution with dilute hydrochloric acid (which will extract the whole of the crude fluoraniline), reducing with zinc, adding ammonia in excess, extracting with ether, and, if necessary, repeating the process. From a specimen of aniline-red prepared by Messrs. Maule and Nicholson, but by what process the author has been unable to learn, as much as 10 per cent. of crude fluoraniline has been extracted. When an ethereal solution of fluoraniline is evaporated spontaneously till all the ether has gone, and then heated on a water-bath to drive off the small quantity of residual water, a strong smell of peppermint is evolved. As the heat is increased, a substance is volatilized which condenses as a dark brown matter insoluble in ether, and as still further heat is applied hydrocyanic acid is evolved.

Fluoraniline is almost insoluble in water when cold, but slightly soluble in hot water, being precipitated as the water cools. It is soluble in dilute hydrochloric, nitric (thus distinguishing it from chrysaniline), sulphuric, and acetic acids, giving fluorescent solutions, is not affected by sulphide of ammonium, and but slightly by hypochlorite of calcium. The alcoholic solution is of a much darker colour than the ethereal, and not so fluorescent; but alcohol added to a solution of fluoraniline in hydrochloric acid increases its fluorescence; it was, indeed, by adding that acid to an alcoholic solution of aniline-red that attention was first drawn to this subject. The fluorescence of this substance is most remarkable. When a beam of sunlight made

* The author has not had an opportunity of examining a new substance exhibiting a green fluorescence, which has recently been obtained by M. Wurtz by a totally different process.

conical by a quartz lens is projected upon a concentrated ethereal solution, all the rays capable of developing fluorescence are absorbed at the surface, so that no cone of light is visible in the solution; but with a dilute solution a brilliant green cone is produced. The colours of the ethereal solution and its fluorescence bear a remarkable resemblance to those of uranium-glass, but with this difference, that when the fluorescent light is examined in the spectroscope, while the fluorescent spectrum of uranium-glass is, as is well known, discontinuous, that of fluoraniline is continuous.

As the investigation of this subject cannot be continued for some time to come, it has been thought desirable to publish the above imperfect note, that other experimenters may have the benefit of the results hitherto obtained.

Hadham House, Upper Clapton,
July 19, 1869.

Postscript, July 21.—Since the above paper was written, the author has discovered, in the aniline-red made from stannic chloride, another fluorescent substance associated with fluoraniline. The fluorescent spectrum consists of red, a very bright green band, and some blue only. To the unassisted eye the fluorescence has a cold blue tint.

XVI. *On the Heating produced in Solid Bodies when they are Sounded. By* Dr. E. Warburg*.

In the twenty-fourth volume of Poggendorff's *Annalen*, William Weber mentions that his attention was excited by the difference which bodies exhibit in the rapidity with which their sound fades away. He shows that the resistance of the air, which must diminish the amplitude the more rapidly the smaller the mass of the body upon which it acts, is inadequate to explain this phenomenon, and he arrives at the conclusion that it must have its origin in the special nature of the substance.

As a matter of fact, the sound of lead fades away more rapidly than that of steel, while the density of lead is far greater than that of steel.

From these considerations, part of the *vis viva* of the vibrations must be consumed in the interior of the sounding body; and the conclusion is obvious that it is here changed into heat. This portion will be greater in the case of those bodies in which, as in lead, the sound rapidly fades away—that is, only impart a small amount of the motion to the surrounding medium.

* Translated from the *Berliner Monatsbericht* for February 1869.

The phenomenon of deadening produced when bodies are connected with other sounding bodies gives rise to similar considerations. If a leaden tube (even a thin one) be so fitted to a glass tube as to form its prolongation, it is found that the longitudinal tone of the glass tube is very considerably deadened. This is the case even if the leaden tube is as long as half a wave-length, in which case the deadening is least. A steel or brass rod produces under these circumstances scarcely any perceptible deadening. These phenomena lead to the assumption that part of the *vis viva* of the vibrations in the interior of the body is consumed—and therefore also to the assumption that by sounding there is a production of heat, and a greater one in lead than in steel.

The author proposed to himself the task of investigating the production of heat by sound from this point of view*. He placed the soldering of a thermopile, in the circuit of which was inserted an astatic galvanometer, against the part to be examined after a body had been made to sound. Before each experiment, he proved that placing the soldering against the body produced no deflection on the galvanometers.

Longitudinal Tones.

He first succeeded in demonstrating the heating produced by sound by means of a bar of wax, the sound of which rapidly fades away. A rod of wax was fixed to a thick glass tube in such a manner that it formed its prolongation; its length amounted to half a wave-length of the note (calculated from the velocity of the propagation of sound in wax, which the author has ascertained, and the detail of which will appear in Poggendorff's *Annalen*). When the soldering of the thermo-element was placed against a node, a deflection of 300 divisions in the direction of heat was produced, while in the loops there was only a deflection of fifty divisions in the same direction.

A leaden tube of 9 millims. external diameter fastened to the glass rod instead of wax, and also as long as half a wave-length, exhibited a heating of 300 to 400 divisions at a node, and of 40 divisions in a loop. A thinner lead tube (4 millims. external diameter) of the same length, connected with the same glass tube, became more strongly heated: a deflection of 600 divisions was obtained when the thermo-element was placed against the node after sounding the tube. The two leaden tubes were then placed end to end at the same end of the glass tube: in this case there was the same heating virtually in both. It is

* It has not hitherto been proved experimentally that heat is produced in solids by sounding; for Sullivan's experiments (Phil. Mag. S. 3. vol. xxvii. p. 261) and Le Roux's (*Comptes Rendus*, vol. l. p. 656) cannot be regarded as a proof of this.

thence to be concluded that a thinner and thicker tube, the amplitude of whose vibrations is the same, develope equal amounts of heat in the unit of section, that in the above experiments the thinner tube was only heated more strongly because the amplitude of the oscillations was greater in it; the latter is also seen in the circumstance that the note of the system is distinctly louder when the thinner is replaced by the thicker tube.

The investigation of self-sounding lead tubes led to the same result. Of three tubes of the same thickness of tubing and length,

A tube of 16 millims. external diameter after brisk rubbing gave no deflection at the node.
A tube of 9 millims. . . . . 200 divisions.
A tube of 4 millims. . . . . 600 „

The intensity of the deflection decreased the greater the distance from the node, and in the loops there was virtually no heating at all. There can be no doubt that here the stronger heating of the thinner tubes is simply explained by the fact that when the same amount of force is used to excite the tone, the amplitudes of vibration in the narrow tubes must be greater than in wider ones; for with wider ones a greater mass is set in motion than with narrower ones.

After it had been thus established what arrangement of the experiment produced the greatest increase of temperature by sounding, it was easy to demonstrate such a heating also in other bodies. The only plan, however, was to connect the metals in the form of thin wire with the sounding body, and then to put this into powerful vibrations. A brass wire $1\frac{1}{2}$ to 2 millims. in diameter, the length of which was half the wave-length of the tone of the glass tube, indicated heat in the node corresponding to 100 divisions. If by shortening the wire the strength of the resonance was increased, 300 divisions were obtained. Then come, in decreasing order of intensity of temperature observed, copper, iron, steel, wood.

A body very remarkable for its deadening properties is caoutchouc; and hence it is not surprising that on a short piece of caoutchouc tubing being fastened to the sounding glass tube a heating of more than 1000 divisions was obtained. A thermometer which, laid on before the experiment, showed 19°, rose, after the sounding, to 21°; the actual increase in temperature amounted therefore to 2°.

While in tubes of other materials when several nodes are formed the increase of temperature on the various nodes is much the same, in the case of caoutchouc this is only perceptible at a small distance from the place where it is fastened on the glass tube.

This is probably due to the sound becoming so enfeebled by its passage through the caoutchouc that its intensity is soon too small to produce an appreciable increase.

The only substance on which I did not succeed in obtaining an increase of temperature by heat was glass. Thin glass tubes put in strong vibration by resonance invariably cracked; and with thick bars I could not observe a development of heat, probably because they could not be put in sufficiently powerful vibrations.

Transversal Tones.

From what has been said, alternate condensations and rarefactions which occur in longitudinal tones are an essential condition for heating to be produced by sound. Since condensations and rarefactions are connected with the bendings which occur in the case of transverse vibrations, an increase in temperature was to be expected in transverse vibrations. This was indeed observed; yet there was a far more complicated distribution of the heat than in the case of longitudinal tones. In producing the tones it is best to use a tuning-fork, and to connect with one leg the body to be investigated in the form of wire or thin tubes, in such a manner that it constitutes the prolongation of the leg in question. It was thus possible to demonstrate an increase of temperature after sounding in the case of caoutchouc, lead, brass, copper, iron, and steel; and the intensity in the various materials corresponded to the values obtained by longitudinal vibrations. Yet in the loops in transverse vibrations there is, after sounding, in general as great an elevation of temperature as on the nodes; in the case of caoutchouc it was certainly ascertained to be greater; only at the free end it was in all cases null. The latter circumstance leads to an explanation of the phenomena. In transverse vibrations the positions of *greatest bending* are those which nearly coincide with the loops, and they are also places where most heat is developed—just as in longitudinal vibrations the places where there is the *greatest change in density*, which coincide with the *nodes*, have also been seen to be places where most heat is developed. In like manner, in accordance with Kundt's experiments*, the action of sounding bars on transmitted polarized light diminishes in the case of longitudinal vibrations towards the *loops*, and in the case of transverse vibrations towards the *nodes* and the *free ends*.

Hence we may sum up the result of this investigation by saying that every solid becomes perceptibly heated by being sounded, provided that sufficiently powerful condensations and rarefactions

* Poggendorff's *Annalen*, vol. cxxiii.

are produced, and that the amount of heat very rapidly increases with the intensity of these condensations and rarefactions.

It has further been established that various bodies exhibit a greater increase in temperature after being sounded the more rapidly their sound fades away, or, what is the same, the more they deaden the sound of other bodies; the greatness of the difference in the increase of temperature observed justifies the statement that the larger increments of temperature do not depend upon a smaller specific heat of the bodies in question, but on the fact that a greater quantity of heat is produced when they are sounded.

In comparing the various bodies as regards the quantity of heat produced by sounding, it is remarked that the production of heat in bodies is greater the smaller the velocity of sound in them; it is greatest in caoutchouc, in which sound scarcely travels 40 metres in a second. This is connected with the fact that the wave-length diminishes with the velocity of sound, and that, if bodies are sounded with the same force, the condensations and rarefactions must be greater in the shorter than in the longer waves.

It is also possible that specific differences in substance may contribute their share to the difference in the production of heat in various bodies.

XVII. *Proceedings of Learned Societies.*

ROYAL INSTITUTION OF GREAT BRITAIN.

May 28, 1869. — On Recent Discoveries in Solar Physics made by means of the Spectroscope. By J. Norman Lockyer, F.R.S.

In the year 1865 two very important memoirs dealing with all the telescopic and photographic observations accumulated up to that time on the subject of solar physics were given to the world. One of them was privately printed in this country; the other appeared in the *Comptes Rendus* of the Paris Academy of Sciences.

I shall not detain you with a lengthened notice of these remarkable papers. I shall merely refer to the explanation given in both of them of the reason that a sun-spot appears dark—the very keystone of any hypothesis dealing with the physical constitution of the sun.

English science, represented by Messrs. De La Rue, Stewart, and Loewy, said that a spot is dark because the solar light is absorbed by a cool, non-luminous, absorbing atmosphere, pouring down there on to the visible surface of the sun, in other words, on to the photosphere.

French science, represented by M. Faye, said that a spot is dark because it is a hole in the photosphere, and the feebly luminous and, therefore, radiating interior gases of the sun are there alone visible.

Now most of you will see in a moment that here was a clear issue, which probably the spectroscope, and possibly nothing else, could solve; for the spectroscope is an instrument whose special *métier* it is to deal with radiation and absorption. It tells us that the light radiated from different bodies gives us spectra of different kinds according to the nature of the radiating body—continuous spectra without bright lines in the case of solids and liquids, and bright lines, with or without continuous spectra, in the case of gases and vapours. It tells us also that absorption dims the spectrum throughout its length when the absorption is *general*, and dims it here and there only when the absorption is *selective*, the well-known Fraunhofer lines being, as you will readily see, an instance of the latter kind. So that we have general and selective radiation, and general and selective absorption.

Now, then, with regard to the English theory, if there were more absorption in a spot than elsewhere, we might expect evidences of absorption; that is, the whole solar spectrum would be visible in the spectrum of a spot, but it would be dimmed, either throughout the length of the spectrum or in places only.

With regard to the French theory, only radiating gaseous matter to deal with, we should, according to the then generally received idea, get bright lines only in the spot-spectrum.

Here then was a tempting opportunity, and one which I considered myself free to use; for, although the spectroscope had then been employed (and you all know how nobly employed) for four years in culling secrets from stars and nebulæ, there was not, so far as I know, either published or unpublished observation on the sun, the nearest star to us. The field was therefore open for me, and I was not entering into another man's labour, when, on the 4th of March, 1866, I attached a small spectroscope to my telescope in order to put the rival theories to a test, and thus bring another power to bear on a question which had remained a puzzle since it was first started by Galileo some two and a half centuries ago.

What I saw I will describe more fully by and by. It is sufficient here to mention that it was in favour of the English theory. There *was* abundant evidence of absorption in the spots, and there *was not* any indication of gaseous radiation.

Having then thus spectroscopically broken ground on the sun, a very natural inquiry was how next to employ this extension of a method of research, the discovery of which Newton had called, nearly two hundred years before, "the oddest, if not the most considerable, detection which hath hitherto been made in the operations of nature."

There seemed one question which the spectroscope should now put to the sun above all others, and it was this:—

"Assuming this absorbing atmosphere to encircle the sun, in accordance with the general idea and Kirchhoff's hypothesis, what are those strange red flames seen apparently in it at total eclipses, jutting here and there from beyond the sun's hidden periphery, and here again hanging cloudlike?"

The tremendous atmosphere, which apparently the spectroscope had now proved to be a cool absorbing one, was supposed to be indicated during eclipses by a halo of light called the "Corona," in which corona the red flames are visible. Now as the red flames are always observed to give out more light than the corona, they were probably hotter than it; and reasoning thus on the matter with my friend Dr. Balfour Stewart one day, we came to the conclusion that they were most probably masses of glowing gas.

Now this being so, the spectroscope *could* help us, and in this way.

The light from solid or liquid bodies, as you all I am sure know, is scattered broadcast, so to speak, by the prism into a long band of light, called a continuous spectrum, because from one end of it to the other the light is persistent.

The light from gaseous and vaporous bodies, on the contrary, is most brilliant in a few channels; it is *husbanded*, and, instead of being scattered broadcast over a long band, is limited to a few lines in the band—in some cases to a very few lines.

Hence, if we have two bodies, one solid or liquid and the other gaseous or vaporous, which give out exactly equal amounts of light, then the bright lines of the latter will be brighter than those parts of the spectrum of the other to which they correspond in colour or refrangibility.

Again, if the gaseous or vaporous substance gives out but few lines, then, although the light which emanates from it may be much less brilliant than that radiated by a solid or liquid, the light may be so localized, and therefore intensified, in one case, and so spread out, and therefore diluted, in the other, that the bright lines from the feeble-light source may in the spectroscope appear much brighter than the corresponding parts of the spectrum of the more lustrous solid body. Now here comes a very important point: supposing the continuous spectrum of a solid or liquid to be mixed with the discontinuous spectrum of a gas, we can, by increasing the number of prisms in a spectroscope, dilute the continuous spectrum of the solid or liquid body very much indeed, and the dispersion will not seemingly reduce the brilliancy of the lines given out by the gas; as a consequence, the more dispersion we employ the brighter relatively will the lines of the gaseous spectrum appear.

The reason why we do not see the prominences every day in our telescopes is that they are put out by the tremendous brightness of our atmosphere near the sun, a brightness due to the fact that the particles in the atmosphere reflect to us the continuous solar spectrum. There is, as it were, a battle between the light proceeding from the prominences and the light reflected by the atmosphere, and, except in eclipses, the victory always remains with the atmosphere.

You will see, however, in a moment, after what I have said, that there was a possibility that if we could bring a spectroscope on the field we might turn the tide of battle altogether, assuming the prominences to be gaseous, as the reflected continuous spectrum might be dispersed almost into invisibility, the brilliancy of the prominence-lines scarcely suffering any diminution by the process.

The first attempt was made in 1866, a Herschel-Browning spectroscope being attached to my telescope; and the first and many succeeding attempts failed: there was not dispersion enough to dilute the spectrum of the regions near to the sun sufficiently, and as a consequence the tell-tale lines still remained veiled and invisible. Nature's secrets were not to be wrested from her by a *coup de main*.

The year 1868 brought us to the now famous eclipse, to see which scientific men hastened from all civilized Europe to India. To this eclipse and its results I need only refer, as they have already been dwelt on at some length in this theatre; suffice it to say that in the eclipse the spectroscope did its duty, and that the gaseous nature of the prominences was put beyond all question.

But there was a magnificent pendant to the eclipse, to which I must request your special attention. One of the observers, M. Janssen—a spectroscopist second to none—the representative, in that peaceful contest, of the Académie des Sciences and of the Bureau des Longitudes, was so struck with the brightness of the prominences rendered visible by the eclipse that, as the sun again lit up the scene, and the prominences disappeared, he exclaimed, "Je reverrai ces lignes-là!" and being prevented by clouds from putting his design into execution that same day, he rose next morning long before the sun, and as soon as our great luminary had risen from a bank of vapours, he succeeded in obtaining spectroscopic evidence of the protuberances he had seen surrounding the eclipsed sun the day before. During the eclipse M. Janssen had been uncertain even as to the number of lines he had observed; but he now by this new method at his leisure determined that the prominences were built up of hydrogen, this fact being indicated by the presence of two bright lines corresponding to the dark lines C and F in the ordinary solar spectrum.

Let me show you how this result was accomplished, by throwing an enlarged photograph of my telescope and spectroscope on the screen. We have first the object-glass of the telescope to collect the sun's rays and to form an image of the sun itself on a screen. In this screen is an excessively narrow slit, through which alone light can reach the spectroscope. This entering beam is grasped by another little object-glass and transformed into a cylinder* of light containing rays of all colours, which is now ready for its journey through the prisms. In its passage through them it is torn by each succeeding prism more out of its path, till at last, on emerging, it crosses the path it took on entering, and enters the little telescope you see, thoroughly dismembered but not disorganized.

Instead now of a cylinder of light containing rays of all colours, we have a cylinder of each ray, which the little telescope compels to paint an image of the slit. Where rays are wanting, the image of the slit remains unpainted—we get a black line; and when the telescope is directed to the sun, so that the narrow slit is entirely within the image of the sun, we get in the field of view of the little telescope a glorious coloured band with these dark lines crossing it.

* Cylindrical, that is, in the case of each pencil.

Of course it is necessary for our purpose to allow only the edge of the sun to fall on the slit, leaving apparently a large portion of the latter unoccupied. What is seen, therefore, is a very narrow band in the field of view of the little telescope and a large space nearly dark, as the dispersion of the instrument is so great that the atmospheric light is almost entirely got rid of, for a reason you are already acquainted with.

Mr. Ladd will now show you on the screen what is seen when the slit reaches a prominence. First a line in the red, very obvious and brilliant, next a more delicate line in the yellow, then another in the green, and two others in the violet; all these lines, with the exception of the yellow line, are in the positions occupied by known lines of hydrogen.

As the height of these bright lines must vary with the height of the prominences, and as the lines will only be visible where there is any hydrogen to depict, it is obvious that the form of the prominences may be determined by confining the attention to one line, and slowly sweeping the slit over it.

The first fruits then of this new method of working with an uneclipsed sun was to tell us the actual composition of the prominences, and to enable us to determine their shapes and dimensions.

For the next steps you must permit me to refer more particularly to my own observations.

When I was first able to obtain results in this country similar to those previously obtained by M. Janssen, though unknown to us, my instrument was incomplete; when other adjustments had been added by Mr. Browning, I found that at whatever part of the sun's edge I looked I could not get rid of the newly discovered lines. They were not so long as I had seen them previously; but there they were, not to be extinguished, showing that for some 5000 miles in height all round the sun there was an envelope of which the prominences were but the higher waves. This envelope I named the "Chromosphere," as it is the region in which all the variously coloured effects are seen in total eclipses, and because I considered it of importance to distinguish between its discontinuous spectrum and the continuous one of the photosphere. And now another fact came out. The bright line F took the form of an arrow-head, the dark Fraunhofer line in the ordinary spectrum forming the shaft, the corresponding chromospheric line forming the head; it was broad close to the sun's edge, and tapered off to a fine point, an appearance not observed in the other lines.

Nature is always full of surprises, and here was a surprise and a magnificent help to further inquiry lurking in this line of hydrogen! MM. Plücker and Hittorf had already recorded that, under certain conditions, the green line of hydrogen widened out; and it at once struck me that the "arrow-head" was nothing but an indication of this widening out as the sun was approached.

I will now, then, for one moment leave the observatory work to say a word on some results recently obtained by Dr. Frankland and myself, in the researches on the radiation and absorption of hydrogen

and other gases and vapours, upon which we have for some time been engaged.

First, as to hydrogen, what could laboratory work tell us about the chromosphere and the prominences?

It was obviously of primary importance—

(1) To determine the cause to which the widening of the F line was due.

(2) To study the hydrogen-spectrum very carefully under varying conditions, with a view of detecting whether or not there existed a line in the orange.

We soon came to the conclusion that the principal, if not the only, cause of the widening of the F line was *pressure*.

Having determined, then, that the phenomena presented by the F line were phenomena depending upon and indicating varying pressures, we were in a position to determine the atmospheric pressure operating in a prominence, in which the red and green lines are nearly of equal width, and in the chromosphere, through which the green line gradually expands as the sun is approached.

With regard to the higher prominences, we have obtained evidence that the gaseous medium of which they are composed exists in a condition of *excessive tenuity*, and that even at the lower surface of the chromosphere (that is, on the sun itself, in common parlance) the pressure is very far below the pressure of the earth's atmosphere.

Now I need hardly point out to you that the determination of the above-mentioned facts leads us necessarily to several important modifications of the received theory of the physical constitution of our central luminary—the theory which we owe to Kirchhoff, who based it upon his examination of the solar spectrum. According to his hypothesis, the photosphere itself is either solid or liquid, and it is surrounded by an extensive cool and non-luminous atmosphere composed of gases and the vapours of the substances incandescent in the photosphere.

We find, however, instead of this compound cool and non-luminous atmosphere outside the photosphere, one which is in a state of incandescence, is therefore luminous, and which gives us merely, or at all events mainly, the spectrum of hydrogen; and the tenuity of this incandescent atmosphere is such that it is extremely improbable that any considerable atmosphere, such as the corona has been imagined to indicate, exists outside it.

Here already, then, we find the "cool absorbing atmosphere" of the theorists terribly reduced in height, and apparently much more simple in its composition than had been imagined by Kirchhoff and others. Dr. Frankland and myself have shown separately:—

(1) That a gaseous condition of the photosphere is quite consistent with its continuous spectrum, whether we regard the spectrum of the general surface or of spots. The possibility of this condition has also been suggested by Messrs. De La Rue, Stewart, and Loewy.

(2) That a sun-spot is a region of greater absorption.

(3) That when photospheric matter is injected into the chromosphere we see bright lines.

(4) That there are bright lines in the solar spectrum itself.

All these are facts which indicate that the absorption to which the reversal of the spectrum and the Fraunhofer lines are due takes place in the photosphere itself or extremely near to it, instead of in an extensive outer absorbing atmosphere. And this conclusion is strengthened by the consideration that otherwise the newly discovered bright lines of hydrogen should themselves show traces of absorption on Kirchhoff's theory; but I shall show you presently that, so far from this being the case, they *appear bright actually in the very centre of the disk*; and, moreover, the vapours of sodium, iron, magnesium, and barium are often bright in the chromosphere, showing that they would always be bright there *if the vapours were always present*, as they should be on Kirchhoff's hypothesis; so that we may say that the photosphere *plus* the chromosphere is the real atmosphere of the sun, and that the sun itself is in such a state of fervid heat that the actual outer boundary of its atmosphere (*i. e.* the chromosphere) is in a state of incandescence.

With regard to the line in the orange I have nothing yet to tell. Dr. Frankland and myself are at the present moment working upon it.

I have next to take you a stage lower into the bowels, not of the earth, but of the sun.

As a rule, the chromosphere rests conformably, as geologists would say, on the photosphere; but the atmosphere (as I have just defined it) is tremendously riddled by convection-currents; and where these are most powerfully at work, the upper layers of the photosphere are injected into the chromosphere. Thus I have observed the lines due to the vapour of sodium, magnesium, barium, and iron in the spectrum of the chromosphere, appearing there as very short and very *thin lines*, generally much thinner than the black lines due to their absorption in the solar spectrum.

These injections are nearly always accompanied by the strangest contortions of the hydrogen-lines, of which more presently. Sometimes during their occurrence the chromosphere seems full of lines, those due to the hydrogen towering above the rest.

At the same time we have tremendous changes in the prominences themselves, which I have recently been able to see in all their beauty. I have attempted to accomplish this in the first instance by means of an oscillating slit; but hearing that Mr. Huggins had succeeded in doing the same thing by means of absorptive media, using an open slit, it struck me at once that an open slit was quite sufficient; and this I find to be the case. By this method the smallest details of the prominences and of the chromosphere itself are rendered perfectly visible and easy of observation, and for the following reason. As you already know, the hydrogen Fraunhofer lines (like all the others) appear dark, because the light which would otherwise paint an image of the slit in the place they occupy is absorbed; but when we have a prominence on the slit, there is light to paint the slit; and as, in the case of any one of the hydrogen-lines, we are working with light of one refrangibility only, on which the prisms have no dispersive power, we may consider the prisms abolished. Further, as we have the

prominence-image coincident with the slit, we shall see it as we see the slit, and the wider we open the slit the more of the prominence shall we see. We may use either the red, or yellow, or green light of hydrogen for the purpose of thus seeing the shape and details of the prominences; how far the slit may be opened depends upon the purity of the sky at the time. I have been perfectly enchanted with the sight which my spectroscope has revealed to me. The solar and atmospheric spectra being hidden, and the image of the wide slit and the part of the prominence under observation alone being visible, the telescope or slit is moved slowly, and the strange shadow-forms flit past and are seen as they are seen in eclipses. Here one is reminded, by the fleecy, infinitely delicate cloud-films, of an English hedge-row with luxuriant elms; here of a densely intertwined tropical forest, the intimately interwoven branches threading in all directions, the prominences generally expanding as they mount upwards, and changing slowly, indeed almost imperceptibly.

It does not at all follow that the largest prominences are those in which the intensest action or the most rapid change is going on—the action as visible to us being generally confined to the regions just in or above the chromosphere, the changes arising from violent uprush or rapid dissipation, the uprush and dissipation representing the birth and death of a prominence. As a rule, the attachment to the chromosphere is narrow and is not often single; higher up, the stems, so to speak, intertwine, and the prominence expands and soars upward until it is lost in delicate filaments, which are carried away in floating masses.

Since last October, up to the time of trying the method of using the open slit, I had obtained evidence of considerable changes in the prominences from day to day. With the open slit it is at once evident that changes on the small scale are continually going on; but it was only on the 14th of March that I observed any change at all comparable in magnitude and rapidity to those already recorded by M. Janssen.

About 9^{h} 45^{m} on that day, with the slit lying nearly along the sun's edge instead of across it as usual, I observed a fine dense prominence near the sun's equator, on the eastern limb, with signs of intense action going on. At 10^{h} 50^{m}, when the action was slackening, I opened the slit and saw at once that the dense appearance had all disappeared and cloud-like filaments had taken its place. The first sketch, now exhibited, embracing an irregular prominence with a long perfectly straight one, was finished at 11^{h} 5^{m}, the height of the prominence being $1' \, 5''$, or about 27,000 miles. I left the observatory for a few minutes, and on returning at 11^{h} 15^{m} I was astonished to find that the straight part of the prominence had entirely disappeared; not even the slightest rack appeared in its place. Whether it was entirely dissipated, or whether parts of it had been wafted towards the other part, I do not know, although I think the latter explanation the more probable one, as the other part had increased.

So much, then, for the chromosphere and the prominences, which

I think the recent work has shown to be the last layer of the true atmosphere of the sun. I shall now invite your attention to spots.

Now, as a rule, precisely those lines which are injected into the photosphere by convection-currents are most thickened in the spectrum of a spot, and the thickening increases with the depth of the spot; so that I no longer regard a spot simply as a cavity, but as a place in which principally the vapours of sodium, barium, iron, and magnesium occupy a lower level than they do ordinarily in the atmosphere.

I have told you before, that when these lines are observed in the chromosphere, they usually are thinner than their usual Fraunhofer lines.

I will now show a photograph of a spot-spectrum on the screen. You will see a black band running across the ordinary spectrum; that black band indicates the general absorption which takes place in a sun-spot. Now mark the behaviour of the Fraunhofer lines; see how they widen as they cross the spot, putting on a sudden blackness and width in the case of a spot with steep sides, expanding gradually in a shelving one. The behaviour of these lines is due to selective absorption.

We have, then, the following facts: mark them well:—

(1) The lines of sodium, magnesium, and barium, when observed in the chromosphere, are among those which are thinner than their usual Fraunhofer lines.

(2) The lines of sodium, magnesium, and barium, when observed in a spot, are among those which are thicker than their usual Fraunhofer lines.

They show, I think, that a spot is the seat of a downrush or downsinking.

Messrs. De La Rue, Stewart, and Loewy, who brought forward the theory of a downrush before my observations of an actual downrush were made in 1865, at once suggested as one advantage of this explanation that all the gradations of darkness, from the faculæ to the central umbra, may be supposed to be due to the same cause, namely, the presence to a greater or less extent of a relatively cooler absorbing atmosphere—thus suggesting as one cause of the darkening of a spot

(1) The general absorption of the atmosphere, thicker here than elsewhere, as the spot is a cavity.

To which the spectroscope added in 1866, as you know,

(2) Greater selective absorption.

I have Dr. Frankland's permission to exhibit an experiment connected with our researches on absorption which will show you that this increased selective absorption can be fairly grappled with in our laboratories. I will show you on the screen the absorption-line due to sodium-vapour, in one part as thin as it is in the ordinary solar spectrum, in another almost if not quite as thick as it appears in a spot; and I accomplish this result in the following way:—Here I have an electric lamp, and by means of this slit I only permit a fine line of light to emerge from it; here the beam passes through a bi-

sulphide-of-carbon prism, and there you see on the screen the glorious spectrum due to the dismemberment of the fine line of polychromatic light. Mr. Pedler will now place a glass tube containing metallic sodium, sealed up with hydrogen, in front of the slit, and will heat it with a spirit-lamp.

As the sodium-vapour rises you see the dark line of absorption make its appearance as an extremely fine line, and finally you see that the light which traverses the upper layer of the sodium scarcely suffers any absorption—the line is thin ; while, on the contrary, the light which has traversed the lower, denser layers has suffered tremendous absorption : the line is inordinately thick, such as we see it in the spectrum of a spot.

So much, then, for the selective absorption. My recent observations, to which I will shortly draw attention, show, I think, that it is of great importance, especially in connexion with the fact that the passage from the penumbra to the umbra is generally less gradual than that from the photosphere to the penumbra. You see now how much is included in the assertion that the photosphere is gaseous.

You are all, I know, familiar with that grand generalization of Kirchhoff's, by which he accounted for the Fraunhofer lines.

If we have a gas or a vapour less luminous than another light-source, and view that light-source through the gas or vapour, then we shall observe absorption of those particular rays which the gaseous vapour would emit if incandescent.

Let us confine our attention to the hydrogen Fraunhofer lines.

When I observe the chromosphere on the sun's limb, with no brighter light-source behind it, I observe its characteristic lines *bright*. But when I observe them on the sun itself (that is, when the brighter sun is on the other side of the hydrogen envelope), then, as a rule, its function is reduced—is toned down ; the envelope acts as an absorber, the lines are observed black.

Now what must we conclude when I tell you that at the present time it is almost impossible to observe the sun for an hour without observing the hydrogen-lines, every now and then, *bright upon the sun itself*?

Not only are the lines observed bright, but it would appear that the strongly luminous hydrogen is carried up by the tremendous convection-currents at different pressures ; and under these circumstances the bright line is seen to be expanded on both sides of its normal position. Moreover at times there is a dim light on both sides of the black line, and the line itself is thinned out, showing that, although there is an uprush of strongly luminous material, the column is still surmounted by some less luminous hydrogen, possibly separated from the other portion, which still performs the functions of an absorber. This seems established by another fact, namely that at times the lines, still black, expand on both sides, as if, in fact, in these regions there was a depression in the chromosphere ; you already know that the pressure is greater at the base of the chromosphere than at the summit.

For this reason it is best to observe these phenomena by means of

the green line, which expands in a more decided manner by pressure than does the red.

I now come to a new field of discovery opened out by these investigations, a branch of the inquiry which I fear you will consider more startling than all the rest—a branch, however, which I have had many opportunities of studying, and which has required me to move with the utmost caution. I allude to the movements of the hydrogen envelope and prominences at which I have before hinted.

Any one who has observed the sun with a powerful telescope, especially in a London fog (all too great a rarity unfortunately for such work), will have been struck with the tremendous changes observed in spots. Now, change means movement; and as spot-phenomena occur immediately below the level of the chromosphere, we may easily imagine that the chromosphere and its higher waves (the prominences) will also partake of the movements, be they up- or downrushes, cyclones, or merely lateral motions. I have thrown on the screen a photograph of a drawing of a sun-spot observed under the clear sky of Rome by Father Secchi—a drawing I regard as a most faithful counterpart of nature.

You see how the photosphere is being driven about and contorted—how here it seems to be torn to ribbons by the action of some tremendous force, how here it is dragged down and shivered to atoms.

The spectroscope enables us to determine the velocities of these movements with a considerable approach to accuracy; and at times they are so great that I am almost afraid to mention them to you.

Let me first endeavour to give you an idea how this result is arrived at; and I must here beg your indulgence for a gross illustration of one of the most supremely delicate of nature's operations.

Imagine a barrack out of which is constantly issuing with measured tread and military precision an infinite number of soldiers in single or Indian file, and suppose yourself in a street seeing these soldiers pass. You stand still and take out your watch and find that so many pass you in a second or minute, and that the number of soldiers as well as the interval between them is always the same.

You now move slowly towards the barrack, still noting what happens. You find that more soldiers pass you than before in the same time, and, reckoned in time, the interval between each soldier is less.

You now move still slowly from the barrack, *i. e.* with the soldiers. You find that fewer soldiers now pass you, and that the interval between each is longer.

Now suppose yourself at rest, and suppose the barrack to have a motion now towards you, now from you.

In the first case the men will be paid out, so to speak, more rapidly. The motion of the barrack-gate towards you will plant each soldier nearer the preceding one than he would have been if the barrack had remained at rest. The soldiers will really be nearer together.

In the second case it is obvious that the interval will be greater, and the soldiers will really be further apart.

So that, generally, representing the interval between each soldier by an elastic cord, if the barrack and the eye approach each other by the motion of either, the cord will contract; in the case of recession, the cord will stretch.

Now let the barrack represent the hydrogen on the sun perpetually paying out waves of light, and let the elastic cord represent one of these waves; its length will be changed if the hydrogen and the eye approach each other by the motion of either.

Particular wave-lengths with the normal velocity of light are represented to us by different colours.

The long waves are red.

The short waves are violet.

Now let us fix our attention on the green wave, the refrangibility of which is indicated by the F line of hydrogen. If any change of wave-length is observed in this line, *and not in the adjacent ones*, it is clear that it is not to the motion of the earth or sun, but to that of the hydrogen itself and alone that the change must be ascribed.

If the hydrogen on the sun is approaching us, *the waves will be crushed together*; they will therefore be shortened, and the light will incline towards the violet—that is, towards the light with the shortest waves; and if the waves are shortened only by the $\frac{1}{10000000}$ of a millimetre we can detect the motion.

If the hydrogen on the sun is receding from us, the waves will be drawn out; they will therefore be longer, and the green ray will incline towards the red.

I must next point out that there are two different circumstances under which the hydrogen may approach or recede from the eye.

I have here a globe, which we will take as representing the sun. Fix your attention on the centre of this globe: it is evident that an uprush or a downrush is necessary to cause any alteration of wave-length. A cyclone or lateral movement of any kind is powerless; there will be no motion to or from the eye, but only at right angles to the line of sight.

Next fix your attention on the edge of the globe—the limb, in astronomical language; here it is evident that an upward or downward movement is as powerless to alter the wave-length as a lateral movement was in the other case, but that, should any lateral or cyclonic movement occur here of sufficient velocity, it might be detected.

So that we have the centre of the disk for studying upward and downward movements, and the limb for studying lateral or cyclonic movements, if they exist.

If the hydrogen-lines were invariably observed to broaden out on both sides, the idea of movement would require to be received with great caution; we might be in presence of phenomena due to greater pressure, either when the lines observed are bright or black upon the sun; but when they widen out, sometimes on one side, sometimes on the other, and sometimes on both, this explanation appears to be untenable, as Dr. Frankland and myself in our researches at the College of Chemistry have never failed to observe a widening out, equally

or nearly so, on both sides of the F line when the pressure of the gas has been increased.

You see now on the screen a diagram showing the strange contortions which the F hydrogen line undergoes at the centre of the sun's disk. Not only have we the line bright, as I have before told you, but the dark one is twisted in places, generally inclining towards the red; and often when this happens we have a bright line on the violet side. You see it sometimes stopping short of one of the small sun-spots, swelling out prior to disappearance, invisible in a facula between two small spots, changed into a bright line and widened out on both sides two or three times in the very small spots, becoming bright near a spot and expanding over it on both sides—very many times widened out near a spot, sometimes considerably, on the less refrangible side, and, finally, extended as a bright line without any thickening over a small spot.

Now the other Fraunhofer lines on the diagram may be looked upon as so many milestones telling us with what rapidity the uprush and downrush take place; for these twistings are nothing more nor less than alterations of wave-length, and, thanks to Ångström's map, we can map out distances along the spectrum from F in $\frac{1}{10000000}$ths of a millimetre from the centre of that line; and we know that an alteration of that line $\frac{1}{10000000}$ millim. towards the violet means a velocity of 38 miles a second towards the eye (*i. e.* an uprush), and that a similar alteration towards the red means a similar velocity from the eye (*i. e.* a downrush). The fact that the black line inclines to the red shows that the less bright hydrogen descends; the fact that the bright line (where both are visible side by side) inclines to the violet shows that the more vivid hydrogen ascends; and the alteration of wave-length is such that 20 miles a second is very common.

Now, observations of the lateral motions at the limb are of course made by the chromospheric bright lines seen beyond the limb. Here the velocities are very much more startling—not velocities of uprush and downrush, as you now know, but swinging and cyclonic motions of the hydrogen.

I will first show you a cyclone observed on the 14th of March; but before I do so let me make one remark. Although the slit used is as narrow as I can make it, let us say $\frac{1}{500}$ (I have not measured it) of an inch, a strip of this breadth, of the sun's image, is something considerable, as the glorious sun himself is painted by my object-glass only about ·94 inch in diameter, so that after all the slit lets in to be analyzed a strip some 1800 miles wide.

Now, suppose we have a cyclone of incandescent hydrogen some 1500 miles wide tearing along with a very rapid rotatory motion, it is clear that all this cyclone could fall within the slit, and that, if the rotatory motion were sufficiently rapid, the spectroscope should separate the waves which are carried towards us from those which are receding. It does this: as you see, we have an alteration of wave-length both towards the red and violet, amounting to something like 40 miles a second. Now it should be clear to you that, by moving the slit first one way and then the other, we may be able to bring it in turn

to such positions that only the light proceeding from either side of the cyclone can enter it. Then we shall have changes of wave-length in one direction only; in each case precisely as you see was observed.

Now let us suppose that instead of a cyclone we have a motion of some portions of the prominence towards the eye, and that, moreover, the rate of motion varies excessively in some portions. What we shall see will be this. The portion of the prominence at rest will give us no alteration of wave-length; its bright line will be in a line with the corresponding black one in the spectrum. The portion moving towards the eye, however, will give us an alteration of wave-length towards the violet. You are now in a position to grasp the phenomena revealed to me by my spectroscope on the 12th instant, when at times the F line was triple! the extreme alteration of wave-length being such that the motion of that part of the prominence giving the most extreme alteration of wave-length must have exceeded 120 miles per second, if we are to explain these phenomena by the only known possible cause which is open to us.

By moving the slit it was possible to see in which part of the prominence these great motions arose, and to follow the change of wave-length to its extremest limit.

By the kindness of Dr. Balfour Stewart I am able to exhibit to you some of the Kew sun-pictures, which show you how these spectroscopic changes are sometimes connected with telescopic ones.

On the 21st of April there was a spot very near the limb which I was enabled to observe continuously for some time. At 7.30 A.M. there was a prominence visible in the field of view, in which tremendous action was evidently going on, for the C, D, and F lines were magnificently bright in the ordinary spectrum itself; and as the spot-spectrum was also visible, it was seen that the prominence was in advance of the spot. The injection into the chromosphere surpassed anything I had seen before, for there was a magnesium cloud quite separated from the limb, and high up in the prominence itself.

By 8.30 the action had quieted down; but at 9.30 another throb was observed, and the new prominence was moving away with tremendous velocity. While this was going on, the hydrogen-lines suddenly became bright on the other side (the earth's side) of the spot, and widened out considerably—indeed to such an extent that I attributed their action to a cyclone, although, as you know, this was a doubtful case.

Now, what said the photographic record? The sun was photographed at $10^h\ 55^m$ A.M., and I hope you will be able to see on the screen how the sun's surface was disturbed near the spot. A subsequent photograph, at $4^h\ 1^m$ P.M. on the same day, shows the limb to be actually broken in that particular place; the photosphere seems to have been absolutely torn away behind the spot, exactly when the spectroscope had afforded me possible evidence of a cyclone!

In connexion with the last branches of the research I have brought to your notice, I may remark that we have two very carefully pre-

pared recent maps of the solar spectrum, one by Kirchhoff, the other by Ångström, made a few years apart and at different epochs with regard to the sun-spot period. If you look at these maps you will see a vast difference in the relative thicknesses of the C and F lines, and great differences in the relative darkness and position of the lines; and if I had time I could show you that we now may be supplied with a barometer, so to speak, to measure the varying pressures in the solar and stellar chromospheres; for, depend upon it, every star has, has had, or will have a chromosphere, and there are no such things as "worlds without hydrogen," any more than there are stars without photospheres. I suggested in 1866 that possibly a spectroscopic examination of the sun's limb might teach us somewhat of the outburst of the star in Corona; and already we see that all that is necessary to get just such an outburst in our own sun is to increase the power of his convection-currents, which we know to be ever at work. Here, then, is one cataclysm the less in astronomy —one less "world on fire," and possibly also a bright light thrown on the past history of our own planet.

I might show you further that we now are beginning to have a better hold on the strange phenomena presented by variable stars, and that an application of the facts I have brought to your notice this evening, taken in connexion with the various types of stars which have been indicated by Father Secchi with admirable philosophy, opens out generalizations of the highest interest and importance, and that, having at length fairly grappled with some of the phenomena of the nearest star, we may soon hope for more certain knowledge of the distant ones.

At present, however, we may well leave speculation for those who prefer it to acquiring facts; let us rather, emboldened by the work which this new method of research has enabled us to accomplish in this country, under the worst atmospheric conditions, in seven short months, go on quietly deciphering one by one the letters of this strange hieroglyphic language which the spectroscope has revealed to us—a language written in fire on that grand orb which to us earth-dwellers is the fountain of light and heat, and even of life itself.

ROYAL SOCIETY.

[Continued from p. 73.]

March 4, 1869.—Lieut.-General Sabine, President, in the Chair.

The following communication was read:—

"Note on the Formation and Phenomena of Clouds." By John Tyndall, LL.D., F.R.S.

It is well known that when a receiver filled with ordinary undried air is exhausted, a cloudiness, due to the precipitation of the aqueous vapour diffused in the air, is produced by the first few strokes of the pump. It is, as might be expected, possible to produce clouds in this way with the vapours of other liquids than water.

In the course of the experiments on the chemical action of light

which have been already communicated in abstract to the Royal Society, I had frequent occasion to observe the precipitation of such clouds in the experimental tubes employed; indeed several days at a time have been devoted solely to the generation and examination of clouds formed by the sudden dilatation of the air in the experimental tubes.

The clouds were generated in two ways: one mode consisted in opening the passage between the filled experimental tube and the air-pump, and then simply dilating the air by working the pump. In the other, the experimental tube was connected with a vessel of suitable size, the passage between which and the experimental tube could be closed by a stopcock. This vessel was first exhausted; on turning the cock the air rushed from the experimental tube into the vessel, the precipitation of a cloud within the tube being a consequence of the transfer. Instead of a special vessel, the cylinders of the air-pump itself were usually employed for this purpose.

It was found possible, by shutting off the residue of air and vapour after each act of precipitation, and again exhausting the cylinders of the pump, to obtain with some substances, and without refilling the experimental tube, fifteen or twenty clouds in succession.

The clouds thus precipitated differed from each other in luminous energy, some shedding forth a mild white light, others flashing out with sudden and surprising brilliancy. This difference of action is, of course, to be referred to the different reflective energies of the particles of the clouds, which were produced by substances of very different refractive indices.

Different clouds, moreover, possess very different degrees of stability; some melt away rapidly, while others linger for minutes in the experimental tube, resting upon its bottom as they dissolve like a heap of snow. The particles of other clouds are trailed through the experimental tube as if they were moving through a viscous medium.

Nothing can exceed the splendour of the diffraction-phenomena exhibited by some of these clouds; the colours are best seen by looking along the experimental tube from a point above it, the face being turned towards the source of illumination. The differential motions introduced by friction against the interior surface of the tube often cause the colours to arrange themselves in distinct layers.

The difference in texture exhibited by different clouds caused me to look a little more closely than I had previously done into the mechanism of cloud-formation. A certain expansion is necessary to bring down the cloud; the moment before precipitation the mass of cooling air and vapour may be regarded as divided into a number of polyhedra, the particles along the bounding surfaces of which move in opposite directions when precipitation actually sets in. Every cloud-particle has consumed a polyhedron of vapour in its formation; and it is manifest that the size of the particle must depend, not only on the size of the vapour polyhedron, but also on the relation of the density of the vapour to that of its liquid. If the vapour were light, and the liquid heavy, other things being equal, the cloud-

particle would be smaller than if the vapour were heavy and the liquid light. There would evidently be more shrinkage in the one case than in the other: these considerations were found valid throughout the experiments. The case of toluol may be taken as representative of a great number of others. The specific gravity of this liquid is 0·85, that of water being unity; the specific gravity of its vapour is 3·26, that of aqueous vapour being 0·6. Now, as the size of the cloud-particle is directly proportional to the specific gravity of the vapour, and inversely proportional to the specific gravity of the liquid, an easy calculation proves that, assuming the size of the vapour polyhedra in both cases to be the same, the size of the particle of toluol cloud must be more than six times that of the particle of aqueous cloud. It is probably impossible to test this question with numerical accuracy; but the comparative coarseness of the toluol cloud is strikingly manifest to the naked eye. The case is, as I have said, representative.

In fact, aqueous vapour is without a parallel in these particulars; it is not only the lightest of all vapours, in the common acceptation of that term, but the lightest of all gases except hydrogen and ammonia. To this circumstance the soft and tender beauty of the clouds of our atmosphere is mainly to be ascribed.

The *sphericity* of the cloud-particles may be immediately inferred from their deportment under the luminous beams. The light which they shed when spherical is *continuous*: but clouds may also be precipitated in solid flakes; and then the incessant sparkling of the cloud shows that its particles are *plates*, and not spheres. Some portions of the same cloud may be composed of spherical particles, others of flakes, the difference being at once manifested through the *calmness* of the one portion of the cloud, and the *uneasiness* of the other. The sparkling of such flakes reminded me of the plates of mica in the river Rhone at its entrance into the Lake of Geneva, when shone upon by a strong sun.

March 11.—Lieut.-General Sabine, President, in the Chair.

The following communication was read:—

"On the Specific Heat and other physical properties of Aqueous Mixtures and Solutions." By A. Dupré, Ph.D., and F. J. M. Page.

Part I.

Mixtures of Ethylic Alcohol and Water.

Section 1. *Specific Heat.*

For the methods employed in estimating the specific heat of these mixtures, see a former abstract, 'Proceedings of the Royal Society,' vol. xvi. p. 336 (Phil. Mag. S. 4. vol. xxxv. p. 464).

In the present paper the authors give the specific heat of an additional number of mixtures, so as to complete the series for every 10 per cent. from water to absolute alcohol.

The following Table gives the mean of the results obtained in all experiments, details of seventy-four of which are given:—

Percentage of alcohol, by weight.	Specific heat found.	Specific heat calculated.	Difference.
5	101·502	....	....
10	103·576	96·043	+ 7·533
20	104·362	92·086	12·276
30	102·602	88·129	14·473
40	96·805	84·172	12·633
45	94·192	82·193	11·999
50	90·633	80·215	10·418
60	84·332	76·258	8·074
70	78·445	72·301	6·144
80	71·690	68·344	3·346
90	65·764	64·387	1·377
100	60·430	....	....

Section 2. *Heat produced by the mixing of Alcohol and Water.*

This was estimated as follows:—The liquid which formed the smallest portion of the mixture was sealed up in a thin glass bulb; this was then introduced into the calorimeter, the glass bulb was broken, the mixture formed, and the rise in the temperature of the calorimeter observed.

The units of heat evolved in the formation of 5 grms. of each mixture were thus calculated, and found to be—

10 per cent. spirit	.... 26·6850	50 per cent. spirit	.... 35·5850
20 ,, ,,	.... 43·9545	60 ,, ,,	.... 27·2620
30 ,, ,,	.... 47·9800	70 ,, ,,	.... 18·8200
40 ,, ,,	.... 44·8630	80 ,, ,,	.... 12·4775
45 ,, ,,	.... 38·8095	90 ,, ,,	.... 7·7025

Section 3. *Boiling-points.*

A small flask was taken; into this 100 cub. centims. of the mixture was introduced, and the mouth of the flask closed by a doubly perforated cork. Into one of these perforations a thermometer was introduced, into the other a bent tube, dipping beneath the surface of the liquid in the flask, and connected at its other extremity with a Liebig condenser. This tube had a lateral opening (inside the flask) just beneath the cork; by means of this the vapour escaped to the condenser, and trickled back into the flask after being condensed. Thus

Percentage of alcohol, by weight.	Boiling-point observed.	Boiling-point calculated *.	Difference.
0	99·4	....	....
10	90·98	97·25	−6·27
20	86·50	95·10	−8·60
30	84·01	92·95	−8·94
40	82·52	90·90	−8·38
45	81·99	89·72	−7·73
50	81·33	88·60	−7·27
60	80·47	86·50	−6·03
70	79·61	84·35	−4·74
80	78·84	82·20	−3·36
90	78·01	80·05	−2·04
100	77·89	....	....

* Calculated on the assumption that the alcohol and water in a mixture have an influence on the boiling-point of the mixture proportional to their respective weights.

the composition of the mixture was retained as uniform as possible. Thus estimated, the barometer standing at 744·4 millims., the boiling-points are given in the preceding Table.

Section 4. *Capillary Attraction.*

This was estimated by carefully observing the heights to which the several mixtures rose in a capillary tube 0·584 millim. in diameter.

These heights were measured by means of a telescope and a millimetre-scale etched on a glass rod. This glass rod was fixed to the capillary tube, and terminated at its lower extremity in a point, which was made just to touch the surface of the liquid.

Several precautions were necessary to render the measurements accurate.

The results are contained in the following Table:—

Percentage of alcohol, by weight.	Height, assuming water = 100 millims.	Relative molecular attraction.	Height calculated.	Difference.
0	100·00	100·00	100·00	....
10	69·17	68·07	93·11	−25·04
20	56·43	54·83	86·22	−31·39
30	48·19	46·15	79·34	−33·19
40	45·30	42·56	72·45	−29·89
45	43·74	40·64	69·00	−28·36
50	42·93	39·43	65·56	−26·13
60	42·30	37·89	58·68	−20·79
70	41·76	36·42	51·79	−15·37
80	41·29	35·03	44·90	− 9·87
90	40·54	33·35	38·02	− 4·67
100	39·21	31·13	31·13	....

The third column gives the length of a column of water equal in weight to the thread of alcoholic mixture contained in the second column, and gives, therefore, a measure of the relative strength of the molecular attraction in the various mixtures.

The experiments were made at a temperature of 16° C.

Section 5. *Rate of Expansion.*

This was determined by estimating the specific gravity of the different mixtures at the temperatures 10° C., 15°·5 C., 20° C.

The specific-gravity bottle has two necks; into one was fitted a thermometer with a long bulb, whilst the other ended in a capillary tube.

This bottle was placed in a water-bath, whose temperature was under perfect control, and thus the specific gravity could be accurately estimated at the above-named temperatures.

Section 6. *Compressibility.*

This property was estimated by an apparatus similar to the one employed by Regnault and Grassi, but of simpler construction.

The piezometer was of glass; pressure was applied to the inside and outside by forcing air into the apparatus by means of a small pump; 0·000002 was always added as a correction for the compressibility of the piezometer.

The two following Tables give the results obtained in Sections 5 and 6.

Percentage of alcohol, by weight.	Volume at 10° C.	Volume at 20° C., found.	Volume at 20° C., calculated.	Difference.
0	100	100·154	100·154	....
10	100	100·212	100·272	−·060
20	100	100·405	100·386	+·019
30	100	100·632	100·498	+·134
40	100	100·783	100·601	+·182
45	100	100·827	100·652	+·175
50	100	100·868	100·700	+·168
59·77	100	100·914	100·789	+·125
69·73	100	100·980	100·874	+·106
79·81	100	101·020	100·954	+·066
89·89	100	101·052	101·034	+·018
100·00	100	101·088	101·088	....

Percentage of alcohol, by weight.	Compressibility for one atmosphere, found.	Compressibility for one atmosphere, calculated.	Difference.
0	0·00004774	0·00004774	....
10	0·00004351	0·00005387	0·00001036
20	0·00003911	0·00005998	0·00002087
30	0·00003902	0·00006584	0·00002682
40	0·00004347	0·00007118	0·00002771
45	0·00004608	0·00007366	0·00002758
50	0·00004878	0·00007600	0·00002722
59·77	0·00005620	0·00008029	0·00002409
69·73	0·00006159	0·00008426	0·00002267
78·81	0·00006942	0·00008775	0·00001833
89·89	0·00007950	0·00009140	0·00001190
100·00	0·00009349	0·00009349	....

Weight of water contained in the piezometer 114·9727 grms.

In conclusion the authors confine themselves to pointing out certain relations which connect the various physical properties examined.

These properties may be divided into two classes, according as they reach a maximum deviation from the theoretical mean at 30 per cent. or 40 per cent.; each of these is divided into two subclasses, one containing those properties in which the numbers found are above those calculated, and the other containing those in which they are below.

Class I.
Subclass *a*. Specific heat.
Heat produced by mixing.
" *b*. Boiling-point.
Capillary attraction.
Class II.
Subclass *c*. Rate of expansion.
" *d*. Compressibility.

Other characters, examined by previous investigators, are :—

1. *Vapour-tension* : this falls under Class I. Subclass *b*.
2. *Specific Gravity.*
3. *Index of refraction.*

The two latter form a new class, coming to a maximum deviation from their theoretical value at 45 per cent.

In subclass *a*, specific heat—by reference to the Tables given, it will be seen that the first addition of alcohol to water (though alcohol has a specific heat much lower than that of water) produces mixtures which have a higher specific heat than water, and that a mixture containing between 30 and 40 per cent. alcohol has the same specific heat as water.

Similarly alcohol, though much more compressible than water, yet, when added to it, forms mixtures less compressible than water; so that a mixture containing between 45 and 50 per cent. alcohol has the same compressibility as water.

The rate of expansion is remarkable, as, starting from water, it at first is below the theoretical value, then rises; at 17 to 18 per cent. the rate of expansion is identical with the calculated expansion; for all mixtures stronger than this, the rate of expansion is constantly above that calculated.

The whole of the physical characters of mixtures of alcohol and water come to a maximum deviation from their theoretical values somewhere between 30 per cent. and 45 per cent. alcohol by weight. The 30 per cent. nearly corresponds to the formula $C_2 H_6 O + 6 O H_2$ ($=29\cdot87$ per cent.); the 45 per cent. has approximately the formula $C_2 H_6 O + 3 O H_2$ ($=46$ per cent.).

Some of the physical properties examined seem to be especially connected with each other; these are:—

1. Specific heat and heat produced by mixing; for by dividing the number of units of heat evolved by 5 grammes of any mixture by 3·411, the elevation of the specific heat of such mixture above the theoretical specific heat is obtained.
2. Boiling-point and capillary attraction; by dividing the depression of the capillary attraction by 3·6, the depression of the boiling-point is obtained.

Deville & Hoek have shown the specific gravity and index of refraction to be connected with each other (Ann. de Chim. et de Physique, 3rd ser. vol. v. Pogg. Ann. vol. cxii.).

Whether the relations thus established between the various physical properties of alcoholic mixtures hold good with other similar substances, or whether these mixtures form a singular exception, must be decided by further research.

GEOLOGICAL SOCIETY.

[Continued from p. 76.]

December 9th, 1868.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—

2. "On the occurrence of Celestine in the Tertiary rocks of Egypt," By H. Bauerman, Esq., F.C.S., and C. Le Neve Foster, D.Sc., F.G.S.

This communication referred to the presence of celestine at two different horizons in the Tertiary escarpment of Mokattam. The beds forming the escarpment may be divided into two parts, namely;—

the upper beds, which are brown, sandy, cellular limestones with numerous oyster-beds; and the lower, or white Nummulitic limestone proper. A bed of marl with fibrous gypsum generally occurs at the junction of the two groups of strata.

In the upper or brown beds celestine occurs with gypsum, sometimes in isolated crystals, but more generally in stellar or spheroidal nodular aggregates, the points of the crystals being turned outwards. About thirty feet lower down in the white limestone, rough irregular crystals of the same mineral are found in open hollows or druses. They are often large, but much decomposed, and apparently crusted with Nummulites, Bryozoa, &c., which are in reality included in the crystals, and have become exposed by erosion. The erosion and alteration of the crystals commences by the roughening of the faces of the prism, owing to the formation of numerous fine striations parallel to the basal planes, and goes on frequently until the entire disappearance of the crystals. The ultimate product is a hollow cast of the crystal, which may then be filled with limestone, forming a pseudomorph by total replacement. This, however, appears to be rare. More generally the dissolved celestine has been redeposited upon the altered crystals, forming macled groups. The secondary crystals are compact, brilliant, and well formed, without included foreign bodies. These phenomena were attributed by the authors to the solubility of sulphate of strontia in chloride of sodium.

3. "Note on the Echinodermata, Bivalve Mollusca, and some other Fossils from the Cretaceous Rocks of Sinai." By Dr. P. Martin Duncan, F.R.S., Sec. G.S., &c.

The author identified the fossils brought by Mr. Bauerman from Sinai as belonging to the Upper-Greensand and Hippuritic-Chalk horizons, and correlated them with those of Algeria and South-eastern Arabia. He determined the following species:—

Heterodiadema Libycum, *Ag. & Desor*, sp.
Discoidea subucula, *Klein.*
——Forguemolli, *H. Coq.*
Epiaster distinctus, *Agass.*
—— tumidus, *Desor.*
Periaster oblongus, *D'Orb.*
Hemiaster Cenomanensis, *Cotteau.*
Phymosoma Delmarrei, *Desor.*
Pseudodiadema variolare, *Brongn.*
Pedinopsis, sp.
Plicatula Fourneli, *H. Coq.*
Pecten asper, *Lam.*
Neithia alpina, *D'Orb.*
Neithia tricostata, *Bayle.*
Exogyra plicata, *Goldfuss.*
Ostrea Auressensis, *H. Coq.*
—— ——, var. major, *Dunc.*
—— Mermeti, *H. Coq.*
Exogyra Overwegi, *von Buch.*
Ostrea Delattrei, *H. Coq.*
—— curvirostris, *Nilss.*
Caprotina Toucasiana, *D'Orb.*
—— subæqualis, *D'Orb.*
—— Archiacianus, *D'Orb.*
Radiolites, sp.
Clavagella, sp.

4. "On the Existence during the Quaternary Period of a Glacier of the Second Order, occupying the 'cirque' of the valley of Palhères in the western part of the granitic 'massif' of the Lozère." By M. C. Martins, For. Corr. G.S.

After mentioning that no one had satisfactorily proved the former existence of glaciers in the Puys of Auvergne, the Cevennes mountains, or the *massif* of the Lozère, the author stated that, from studying the Government map, it occurred to him that traces of a glacier ought to be found in the eastern part of the granitic massif of the

Lozère, at the upper portion of the Valley of Palhères, which opens near Villefort. An examination of the district in question proved the former existence of a glacier which was limited to the cirque which enclosed it, and did not descend into the valley. A lateral and a terminal moraine were found, and *roches perchées* were observed on the sides of the valley. No striæ or polished surfaces were seen, owing to the schistose rocks being easily decomposed.

XVIII. *Intelligence and Miscellaneous Articles.*

ON THE COMPRESSIBILITY OF LIQUIDS.

BY MM. AMAURY AND DESCAMPS.

IN June 1868, in conjunction with M. Jamin, we laid before the Academy a method for measuring the compressibility of liquids; since then M. Jamin has intrusted to us the task of continuing this research. We have made a great number of determinations, the results of which we have the honour to lay before the Academy. The following Table gives the coefficients of compressibility for one atmosphere:—

Distilled water at	15° C.	0·0000457
Alcohol	0	0·0000835
Alcohol	15	0·0000911
Ether	0	·0·000109
Ether	14	0·000128
Sulphide of carbon	14	0·0000635
Mercury	15	0·00000187

Solution of chloride of potassium,—

Containing in 1000 of water	50 of KCl	0·0000419
,, ,,	100 ,,	0·0000388
,, ,,	150 ,,	0·0000556
,, ,,	200 ,,	0·0000332
,, ,,	250 ,,	0·0000318
,, ,,	300 ,,	0·0000306
Water		0·0000457

These coefficients have been deduced from experiments in which the pressure varied from 1 to 10 atmospheres.

We may observe that the coefficient 0·00000187 found for mercury varies considerably from the coefficient 0·00000295 which Grassi obtained by the use of M. Regnault's method, while with the more compressible liquids the agreement between our numbers and those of M. Grassi is perfect. This difference arises from the circumstance that, as the compressibility of mercury is very small, the least error in the measurement of the correction due to the change of volume in the piezometer has a considerable influence on the true coefficient, whereas with the more compressible liquids this source of error is less apparent.

The expansion of liquids, as is well known, gradually increases with the temperature, and, when they reach the boiling-point, is virtually equal to that of gases. We imagined it would be the same with their coefficient of compressibility, and we made very accurate experiments with water, alcohol, and ether from this point of view. We measured the coefficient of compressibility under very feeble

pressures (only about a centimetre higher than the maximum tension of these liquids), but we were unable to recognize any change in the value of the coefficients of compressibility.—*Comptes Rendus*, June 28, 1869.

MEASUREMENT OF THE ELECTRICAL CONDUCTIVITY OF LIQUIDS HITHERTO SUPPOSED TO BE INSULATORS. BY M. SAÏD-EFFENDI.

M. Jamin has desired me to execute a method which he devised for electrolyzing liquids of small conducting-power. The experiments were made in the laboratory of the Sorbonne under his direction. The method is as follows:—

The quantity of electricity which passes through a conductor is proportional to its conductivity and its section, but is inversely as its length. If the length be diminished and the section increased, a current may be passed even through substances supposed to be insulators. In the case of liquids this is effected by superposing two large plates of platinum, kept apart by flannel or silk or glass, and coiling them round a tube; then, after being connected with the poles of a battery, they are immersed in a voltameter. They thus represent a conductor, the length l of which is the thickness of the material which separates the plates, and the section is twice their surface $2s$. In the present experiments l was about a millimetre, and $2s$ amounted to 195,000 square millimetres. When the roll was immersed in a liquid the conductibility of which was c, the resistance was equal to $\frac{l}{2s}\frac{1}{c}$, or to $\frac{1}{195000}\frac{1}{c}$. It was as if the conductivity had become about two hundred thousand times as great.

By this means even the worst-conducting liquids are readily traversed by the current. The following are the principal facts which have been observed:—

(1) Distilled water disengages with four Bunsen's elements as much gas as acidulated water in an ordinary voltameter. It is therefore an electrolyte. But it becomes heated; for a portion of the gases recombines on the surface of the platinum. The volume of gas is thus less with this pure water than with a voltameter containing acidulated water placed in the circuit. As the intensity diminishes the difference increases, and when the current is very weak there is no apparent decomposition in the apparatus.

(2) It is only when subjected to the action of powerful batteries that alcohol has hitherto afforded signs of decomposition, which might be attributed to the presence of foreign substances. With our apparatus four elements disengage considerable quantities of hydrogen, mixed with a small quantity of oxygen.

(3) Oil of turpentine conducts far worse; eight Bunsen's elements are necessary to produce a distinct decomposition.

(4) Rectified oil of petroleum is decomposed with great ease. The gas collected is inflammable, and during its combustion it deposits carbon upon the sides of the belljar in which it is contained. This deposit may be due to the presence of petroleum-vapour in the liberated gas.

Further researches will give us the composition of the products disengaged during these experiments. I have been especially engaged in measuring the conductivity of these various liquids.

I passed the current through the apparatus and through a tangent-compass, which at the first moment indicated an intensity i. The apparatus was then removed and replaced by coils of known resistance, and by a rheostat the length of which could be varied so as to reproduce the intensity i. The resistance of the liquid was equal to that of the coils and of the rheostat.

The numbers obtained are the following; they are inversely as the conducting-power of the liquid:—

Liquids.	Turns of rheostat.	Conductivity.
Water	55	1000
Petroleum	765	72
Sulphide of carbon	1000	55
Alcohol	1130	49
Ether	1375	40
Oil of turpentine	2380	23
Benzole	3480	16

—*Comptes Rendus*, June 28, 1869.

ON THE HEAT DEVELOPED IN DISCONTINUOUS CURRENTS. BY MM. JAMIN AND ROGER.

Pouillet has shown that when a current of the intensity I is passed into a short rectilinear circuit which developes no phenomena of induction, and which is broken at very short and regular intervals by a vibrating apparatus, the tangent-compass exhibits an apparent intensity I_1. This intensity is equal to I diminished in the ratio of the time α, during which the current passes, to the duration I of one vibration of the break, so that we have

$$I_1 = I\alpha.$$

We may infer from this result that the broken current is made up of successive fragments of currents which last during the time α, and which have a real intensity I, and that there is no change either at the moment of making or of breaking each.

On the other hand, we know that, according to Joule, the amount of heat, C, disengaged in the unit of time in each resistance r, by a current having the intensity I, is proportional to this resistance r and to the square I^2 of this intensity; it is equal to KrI^2, K being a constant. This law has been found to hold good for continuous currents; we have investigated whether it holds in the case of broken currents.

For this purpose we passed these currents through a thermorheometer, an instrument which one of us devised, and which was laid before the Academy on the 6th of July 1868. It consists essentially of a fine platinum wire, the length of which may be varied, and which is immersed in the reservoir of a thermometer in the middle of an isolating liquid. The heat developed by the current is transmitted to this liquid, and is measured by the expansion observed. Operating in this manner, we have ascertained that broken currents always develope more heat than continuous currents of the same apparent intensity, I_1.

This fact does not contradict Joule's law; we shall, on the contrary, see that, when generalized, it justifies the ideas of Pouillet. For

according to this physicist, each section of the current, having a real intensity I and a duration α, must disengage during a vibration a quantity of heat equal to $KrI^2\alpha$. If the real intensity I is replaced by its value $\frac{I_1}{\alpha}$, the heat should be $\frac{KrI_1^2}{\alpha}$. Other things being equal, it will be a minimum when $\alpha=1$, that is, when the current is continuous; it will increase when α diminishes, that is, when the duration of each fragment of a current decreases.

To verify this theoretical formula we used an ordinary Froment's break. A platinum point fitted to a vibrating spring, on sinking, dipped into a mercury-cup and transmitted the current; it emerged from it as it rose, and broke the current. The duration of each fragment was prolonged by raising the level of the mercury, and was diminished by lowering it; the value of α (that is, the duration of the immersion) was easily measured.

The following Table shows:—(1) that I_1, the apparent intensity of the broken current, may be calculated by Ohm and Pouillet's law, and that it is equal to $\frac{A\alpha}{R+r}$, A being the electromotive force, and $R+r$ the total resistance of the circuit; (2) that the quantity of heat developed in the resistance r, divided by $\frac{rI_1^2}{\alpha}$, is a constant quantity equal to K ($K=0{\cdot}19$), whether the current is broken or whether it is continuous.

TABLE I.—Values of K and of I_1 without Coil.
($A=410{\cdot}8$, $R=3{\cdot}65$.)

Intensity I_1.		Resistance r.	$\alpha=1$.		$\alpha=0{\cdot}06$.	
Observed.	Calculated.		C.	$K=\frac{C\alpha}{rI_1^2}$.	C.	$K=\frac{C\alpha}{rI_1^2}$.
14·40	14·20	25·30	1080	0·20	1620	0·18
15·45	15·10	23·62	1160	0·20	1710	0·18
16·55	16·63	21·04	1150	0·20	1838	0·19
18·90	19·40	18·12	1120	0·20	2118	0·19
21·43	21·23	15·70	1470	0·20	2120	0·19
24·16	24·25	13·25	1640	0·21	2520	0·19
28·72	28·82	10·66	1800	0·20	3820	0·20
35·60	35·35	7·97	2150	0·21	3510	0·20
44·70	45·29	5·42	2490	0·23	4150	0·20
			Means...	0·20	......	0·19

It is known that matters are not so simple when there is placed in the circuit a coil containing soft iron; the apparent intensity of the discontinuous current is not given by the formula $I_1=I\alpha$; it is far smaller, and follows new laws now well known and investigated by several physicists. Let us denote it by I'_1; it is obvious that then each fragment of the current is very complicated—enfeebled at the outset by the counter-current, and increased when it is broken by the final shock (the extra current). It was probable that Joule's law would be modified in a thermorheometer placed in the circuit.

This was not so; the quantity of heat disengaged in this thermorheometer was always represented by the formula KrI'^2_1, at least when the breaks were rapid enough, just as if each section of the current had a real constant intensity $\frac{I'_1}{\alpha}$; I'_1 was determined by the special action of the coil according to new laws, which are not those of Ohm.

This is shown by the following Table, obtained as the result of experiments where a coil was interposed in the circuit.

TABLE II.—Values of K with a Coil in the Circuit.

Intensity I'_1.	Resistance r.	$\alpha=1$.		$\alpha=0.5$.	
		C.	$K=\frac{C\alpha}{rI'^2_1}$.	C.	$K=\frac{C\alpha}{rI'^2_1}$.
9	25·46	440	0·20	647	0·19
9·25	23·88	350	0·17	755	0·17
9·92	21·01	376	0·18	845	0·21
11·00	18·41	381	0·17	1039	0·23
12·52	15·78	466	0·18	915	0·18
13·90	13·15	426	0·18	997	0·21
15·65	10·57	427	0·16	970	0·18
18·70	7·86	476	0·17	1014	0·19
22·50	5·29	467	0·16	965	0·17
23·83	3·37	289	0·15	791	0·20
25·95	1·81	265	0·21	611	0·25
		Means...	0·18	......	0·19

But if there is no change in that portion of the circuit which is made up of the thermorheometer (that is, in the portion where there is no induction), all is modified in the coil; and if its resistance is R, the heat there produced is far more than that calculated by the formula $\frac{KRI'^2_1}{\alpha}$. The law has therefore been changed during the induction of a current upon itself in that portion of the circuit where this induction takes place; but it is only changed in this portion. We shall, before long, investigate this change.

We may be permitted to advert to a claim of priority which M. Le Roux has made.

M. Le Roux published in 1857 some purely theoretical ideas, according to which a fragment of a current would meet in every portion of the conductor a resistance greater than the statical resistance which Ohm's laws assign to this conductor; and in our preceding experiments he has seen a confirmation of his ideas.

We are the more at a loss to understand this reclamation because our formulæ are in entire disagreement with those of M. Le Roux, and because, far from having justified his theory, we think we have proved that it has no foundation.

In this investigation we prove that the basis of his reasoning is inexact, and that a broken current acts in a rectilinear circuit like a continuous current. True, things are far more complex in a coil; but that is a case of pure induction, as Helmholtz has proved. —*Comptes Rendus*, March 22, 1869.

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[FOURTH SERIES.]

SEPTEMBER 1869.

XIX. *On the Construction of the Galvanometer used in Electrical Discharges, and on the Path of the Extra Currents through the Electric Spark.* *By* E. Edlund*.

I.

When an electric discharge is passed through a galvanometer in which the individual coils are well insulated, it frequently happens that the position of equilibrium of the needle is altered, and that this alteration lasts even after the discharge. This disadvantage may be greater or less, according to the construction of the galvanometer and the distance from the coils to the moveable parts of the instrument, while the quantity and density of the discharged electricity moreover exert great influence in this respect. If the electrical discharges are very powerful, it may happen that the galvanometer becomes quite spoiled for accurate determinations of the discharge.

There are several causes for this imperfection of the instrument in question. It is well known that strong discharges can bring about a change in the distribution of magnetism in the magnet. The electrical shock can make the magnetic distribution stronger or weaker, or even invert the poles, or change the line of connexion between them. If the galvanometer has an astatic system, the electrical shock may easily alter the ratio of the strength of the magnetisms in the two needles, by which the delicacy is altered, and sometimes a change ensues in the position of equilibrium of the system of needles. Hence a galvano-

* Translated from Poggendorff's *Annalen*, No. 3, 1869.

meter for electrical discharges cannot well be provided with an astatic system: a single needle must be used; and to make it more delicate, either a portion of the directive force of the earth's magnetism must be compensated by external magnets, or, what is better, a mirror with telescope and scale may be used. If in using a single needle the suspending thread had no tendency to torsion, the delicacy of the instrument would be independent of the strength of the magnetism in the needle; for the directive force of the needle in this case would increase or decrease in the same ratio as the action of the current upon it. The position of equilibrium of the needle would also be independent of the strength of the magnet, provided the position of the magnetic axis in the needle were unchanged. The position of equilibrium is also unchanged if the strength is increased or diminished, provided the force of torsion of the thread only tends to bring the needle into the magnetic meridian. Hence, in order that the instrument may retain as far as possible its delicacy, and moreover not have its position of equilibrium altered by changes in the strength of the magnetism which powerful electrical discharges may cause, the directive force which the suspending thread exerts on the needle in virtue of its torsion must be small as compared with the action of the earth's magnetism, and the position of equilibrium caused by torsion must coincide with the magnetic meridian. According to Professor Riess*, the magnetism of the needle is greatly protected if between it and the coils there is a thick copper sheath, which at the same time acts as a damper in bringing the oscillating needle quickly to rest.

But it is easy to see that the action of the electricity on the magnet is not the sole or even the principal cause of the change in the position of equilibrium which results from the passage of the electrical discharge through the coils of the galvanometer. The galvanometer which I used in my former experiments on the electromotive force of the electrical spark had a single needle, which was firmly connected with a mirror, by the aid of which the deflections were read off by the telescope and scale in the ordinary manner. The mirror consisted of glass, and the back was covered with a thin metal disk. The galvanometer-wire, which consisted of copper, was 1 millim. in diameter, and was surrounded by a coating of gutta percha 2 millims. thick. Hence the entire thickness of the wire, including the insulating coating, was 5 millims. This wire was wound in forty coils round a mahogany frame. The aperture in the frame, in which the magnetic needle was suspended by a cocoon-thread, was 50 millims. in length by 30 in height. The length of the needle was 42 mil-

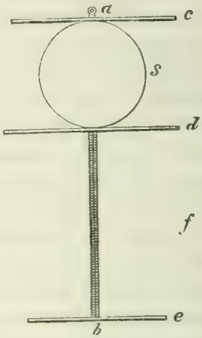
* Abhandlung: "Zu der Lehre von der Reibungs-Electricität," Berlin, 1867, p. 314.

lims. The mirror was above the frame which was surrounded by the wire; and the whole was protected by a bell-jar. When the magnetic needle was removed and replaced by a brass needle of the same size, and the mirror with its affixed needle was suspended by two cocoon-threads, by which the moveable system obtained a definite position of equilibrium, it was observed that this position of equilibrium was altered when a strong discharge was passed through the galvanometer-wire. The alteration in the position of equilibrium could not be due to a change in the magnetism of the needle; for there was no magnetic needle in the apparatus. When the glass globe was carefully removed and the mirror investigated, it was found to be electrical. This alteration in the position of equilibrium was thus due to the fact that in the discharge electrical induction was produced in the moveable parts of the instrument, which acted electroscopically on the fixed parts and produced an altered position of equilibrium.

Hence the moveable parts had to be constructed in such a manner that the electrical action between them and the fixed parts should be unable to turn the moveable system about its own axis. It is clear that if the moveable body suspended by a cocoon-thread were bounded by a surface of rotation the axis of which were the prolongation of the cocoon-thread, and if the surface were made of a conducting material, the electroscopic action between this body and the fixed parts of the instrument could not effect any rotation about the axis in question. If electricity of either kind has collected upon any place (for instance on the gutta-percha-covered wire), this induces electricity in the body in question: the electricity of the opposite kind collects in the point nearest to the fixed attracting point, and the other electricity is driven to the furthest. But if the body is bounded by a surface of the kind mentioned, it is readily seen that the line of junction between the fixed point of action and the two corresponding points upon the moveable body will go through the axis of rotation, and there can thus be no rotation. All that could possibly happen is, that the system would be attracted a little on one side, so that the axis of rotation would no longer be vertical; but no rotation can be thereby produced, provided the centre of gravity of the system lies in the axis. But since a plane mirror is necessary for reading off, the moveable system cannot have the form in question. I have accordingly endeavoured to obtain this object in the following manner:—

The glass mirror which I previously used was exchanged for a round plane-polished silver mirror, the diameter of which was 30 millims. The object of this was to remove the non-conducting glass. Both above and below this mirror, and in direct contact with it, a horizontal circular disk of thin metal foil was placed.

Both disks were of the same size (that is, 50 millims. in diameter); and the axis of rotation of the system, when suspended by the cocoon-thread, went through the centre of each. The magnetic needle was let into a circular copper disk in such a manner that the upper sides of the needle and of the disk lay in the same plane, and their centres coincided. The disk and the needle were soldered together so as to produce perfect conduction between them. The centre of this disk was now made to coincide with the axis of rotation, so that it became horizontal; the adjacent figure renders this arrangement more intelligible. *s* is the silver mirror, *a b* the round metal rod which constitutes the axis of rotation, and *c*, *d*, and *e* are the circular disks, in the latter of which the magnetic needle is inserted. The coils of the galvanometer surround the disk *e* and pass between *d* and *e*, so that the disks *d* and *c* and the mirror are at the top. If, now, in the discharge electricity remains upon any point, for instance at *f*, in the coils, it is clear that it can produce no rotation in consequence of its influence on the disks *c*, *d*, or *e*. Of the electricity which is produced in the mirror *s* in consequence of induction, one part is repelled to the disk *c*, and the other attracted to the disk *d*, and both thereby become innocuous.



On testing, it was evident that this arrangement has a decided advantage over that previously employed. In my former experiments, a Leyden jar charged to saturation could not be discharged through the galvanometer without producing a material change in the position of equilibrium. When one of the galvanometer-wires was directly connected with one of the combs of a Holtz's induction-machine, and the other ended with a knob in the vicinity of the other comb, so that while the machine was at work sparks sprang across, in my previous experiments a considerable alteration was produced in the position of equilibrium after the action of the machine had ceased. Hence to avoid this a shunt was used between the conducting-wires, so that only a small portion of the shock traversed the galvanometer.

In the new arrangement of the moveable part of the galvanometer this bridge was quite superfluous, and the entire discharge could pass through the galvanometer. There was indeed an alteration in the position of equilibrium if the discharges were particularly strong; but it was not so great as to act injuriously on the accuracy of the measurements, and still less to render them impossible. When one galvanometer-wire was connected

with one comb of the induction-machine and the other was free and insulated, so that the galvanometer-wire became saturated with electricity while the machine was at work, there was a material alteration in the position of equilibrium. But this alteration disappeared immediately one of the wires was put in connexion with the earth. These preliminary experiments were made partly when the metallic disk on which was the magnetic needle was firmly screwed to the axis of rotation, and partly when this metal disk was removed and instead of it another metal disk of equal size, but without a magnet, was fixed to the axis, in which latter case the system attained its position of equilibrium by a bifilar suspension from two cocoon-threads. As the experiments gave the same result in both cases, the alteration in the position of equilibrium must have been due to some electroscopic cause. When the two round disks *c* and *d* were removed, experiment showed that the changes in the position of equilibrium became considerably greater; hence the disks performed their expected service. That the galvanometer with the new arrangement of the moveable parts was not quite unaffected by very strong discharges was doubtless due to the moveable system being somewhat obliquely attracted by the electrical action; so that the axis of rotation cannot have hung quite vertically. If in this case everything is not accurately centred, so that the centre of gravity lies in the axis of rotation (which is very difficult, if not impossible), it is clear that a change in the position of equilibrium must ensue. Seeing that electroscopic phenomena may under certain circumstances so closely resemble magnetic ones that a confusion between them is possible, before a galvanometer is used for actual measurements we must satisfy ourselves that under the present circumstances no electroscopic actions occur.

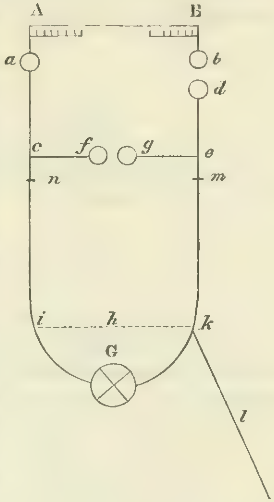
II.

When a closed conducting-wire is in the vicinity of the circuit of an electrical battery, an electric current is produced in the former when the battery is discharged through the latter. This secondary current in the conducting-wire is stronger the longer the portions of the wires which act upon each other. Hence, in order to obtain strong inductive actions, the wire and the circuit must be coiled spirally near to one another. These currents were discovered almost simultaneously by Henry, Marianini, and Riess. A similar inductive action is also produced if the circuit at one part consists of two branches, one of which is long and coiled as a spiral. In the discharge of the battery, which in this case partially traverses both branches, an induction-current is formed in the spiral, which discharges itself through the other branch. Baron Wrede has shown from theoretical consi-

derations that, like those resulting from voltaic induction, these currents are formed of two currents equal in quantity, one of which has the same and the other the opposite direction to that of the primary current*. As these currents are equal in quantity, and in opposite directions, they cannot deflect the magnetic needle; but they can disengage heat, and, as their intensities may be unequal, can also produce magnetic induction in hardened steel needles. This view as to the nature of the induction-currents in question, which rests upon theoretical considerations, has been confirmed since the discovery of the electrical valve has furnished an unfailing means of distinguishing between the two opposed currents. The electrical valve consists of a hollow glass cylinder in which air is rarefied at pleasure. One end of this is closed air-tight by a glass disk; and at the other end is a brass cap with a stopcock, by which it can be connected with an air-pump. Through the glass disk passes a platinum wire, of which one end is level with the inner surface of the glass disk, and the outer end can be connected with a conducting-wire. Inside the cylinder a brass rod extends from the brass cap; the rod terminates in a brass disk, which is parallel with, and at a short distance from, the glass disk. When the air is adequately exhausted, and the platinum wire connected with one and the brass cap with the other end of the induction-spiral, it is proved that only one of the two induction-currents can traverse the valve; for Riess found that when a galvanometer is placed in the circuit, the magnetic needle gives a deflection in a direction which differs according as one or the other end of the induction-spiral is connected with the platinum wire†.

In my investigation on the electromotive force in the electrical spark, there was no other spiral in the circuits than those which were formed by the forty coils of the galvanometer‡. In this spiral induction-currents were of course formed when the electrical discharge traversed them; but it is readily seen, from the manner in which the experiments were arranged, that these induction-currents could have no influence upon the deflection of the magnetic needle. In the adjacent figure, A B represents the rotating induction-disk, and *a b* the two combs. An insulated copper wire, *a c*, was directly connected with *a*, whereas the insulated wire *d e* terminated in a brass knob *d* in the neighbourhood of *b*. From *c* and *e* insulated conducting-wires passed to the knobs *f* and *g*. Two other conducting-wires went from the points *c* and *e* to the galvanometer G. At *m* a rheostat was inserted, consisting of an insulated thin German-silver wire. Be-

* Berzelius, *Jahresbericht*, vol. xx. p. 119.
† Pogg. *Ann.* vol. cxx. p. 513.
‡ Ibid. vol. cxxxiv. p. 337. Phil. Mag. S. 4. vol. xxxvii. p. 41.



tween the points i and k was a bridge of German-silver wire; and the point k was moreover connected by the conducting-wire l with the water-pipe in the house, and was thus placed in conducting communication with the earth. When the disk A B was rotated, sparks passed between b and d as well as between f and g, and the needle made a deflection. The resistance in the wire h was infinitely small, compared with that of the rheostat m and in the spark between f and g. Hence the induction-currents formed in the coils of the galvanometer passed almost exclusively through the bridge h; and as they were equal in quantity while opposite in direction, their action upon the needle was of course imperceptible. This would not have been the case if the bridge h had not existed, and the currents had had to pass through the spark between f and g; for this, as will afterwards be shown, acts like an electrical valve—that is, transmits one current but stops the other. Polarization-experiments showed, moreover, that the current obtained arose from the spark between f and g, and not from the induction of the discharge-current in the galvanometer-coils; for in these experiments the galvanometer was removed, and there was no other spiral in the conductions; so that there could be no induction. After the galvanometer, as previously shown, had been so much improved that the bridge h could be removed without disadvantage, I investigated more closely the phenomena in question; and as the results obtained seem to offer some interest, I will give them here.

At the time the galvanometer was made I also had a coil constructed for making induction-experiments, which in all respects was like the coil of the galvanometer. The wooden frame had the same dimensions; the wire covered with gutta percha was of the same kind; and the number of windings in both coils was the same, namely forty. Hence under the same circumstances both coils must exert the same inductive actions. If the voltaic resistance in the rheostat m was called 100, it was found that the resistance in each of the coils was 4·5, and the resistance in the two conducting-wires from the points c and e to the galvanometer amounted to about as much. The following experiments were made with this coil, which in the sequel will be called R :—

Experiment I.—The bridge h was removed, so that the entire discharge traversed the galvanometer :—

	52·5
	50·5
	49·0
Mean . .	50·7

The coil R was thereupon interposed between e and m, and the deflections obtained were

	30·1
	27·8
	27·4
	27·6
Mean . .	28·2

When R was placed towards n on the opposite side, there was obtained

	28·3
	28·1
	27·9
Mean . .	27·9

The coil R was then removed, and, in order to ascertain if there had been any change in the induction-machine, the first experiments were repeated. The following deflections were observed :—

	51·2
	50·2
	46·2
Mean . .	49·2

The mean of the first and last experiments is 50·0, and that of the middle ones 28·1. Hence the induction-currents in the coil R had diminished the deflection of the magnetic needle by 21·9 divisions. Of these induction-currents, one had the opposite and the other the same direction as that of the discharge. The first may be designated as A, and the latter as B. Hence in these experiments the currents A traversed the spark between f and g more easily than the currents B. The spark accordingly acts in this case like an electrical valve.

Experiment II.—This experiment was made in order to investigate the action of induction-currents upon the deflection when R was interposed between g and e. The currents now traversed R in the opposite direction to the former one. When no coil was interposed in the conduction the following deflections were observed :—

	40·5
	42·0
Mean . .	41·3

R was inserted between g and e, by which there was obtained

	27·8
	27·8
Mean . .	27·8

After removing R there was once more observed

	40·3
	41·3
Mean . .	40·8

In this case also the deflection was diminished by the induction-currents. It is easily ascertained that it was the currents B which traversed the spark between f and g with greater facility. By introducing the coil R into the circuit the resistance was a little increased. To convince myself that this was not the cause of the diminution in the deflection of the magnetic needle, a few experiments were made in which the resistance of the rheostat when R was interposed was so much diminished that the total resistance was a little less than when R was removed. But these experiments gave just the same results as the above. The small alteration in the resistance had therefore no perceptible influence upon the result obtained. The experiments were made in such a manner that the place where the spark was formed was removed from the position indicated by the figure, a little towards n, while the rheostat took its place between c and e. But in this case also the deflection was lessened in the same manner as before by the induction-currents.

It may at first sight appear unexpected that in one case the currents A, but in the other the currents B, should be able more easily to traverse the spark. Yet closer consideration shows that, in one view, A in the first and B in the second experiment have a common character, upon which some stress must here be laid. In the first case it is the current A which traverses the spark in the same direction as the electrical discharge, while in the second it is the current B. It follows hence, *that of the induction-currents formed by electro-induction, those which endeavour to traverse the spark in the same direction as the discharge also penetrate it most readily.*

That the induction-currents which are formed in the coil of the galvanometer itself also diminish the deflection of the needle, necessarily follows from what has preceded, and scarcely needs any proof. Yet it was very easy to demonstrate this experimentally in the following manner. In front of the galvanometer a German-silver wire was inserted between the points i and k, the resistance of which was thrice that of the resistance in the coil of the galvanometer. Hence, of the currents which

arose in the electric spark, only three-quarters traversed the galvanometer. If there is a bridge between i and k, the resistance of which is small as compared with the resistance in the spark and in the rheostat m, the greatest part of the induction produced in the galvanometer passes through the bridge; and as they are equal in quantity and opposite in direction, their action on the magnetic needle is eliminated. But if the bridge is removed, the induction-currents act upon the magnetic needle.

If, now, this action is in the opposite direction to that which is caused by the spark, the deflection on inserting the bridge must be more than three-fourths of that which ensues when the bridge is removed.

The following experiments show that the first deflection is even considerably greater than the latter.

Experiment III.—The bridge inserted between the points i and k. There were thus obtained the following deflections when the machine was at work:—

	Divisions.
	24·0
	26·0
	25·5
Mean . .	25·2

Without the bridge the deflections were

	13·3
	12·3
	13·3
	12·8
Mean . .	12·9

The bridge was again introduced, upon which the deflections became

	26·2
	23·2
	23·7
	20·7
Mean . .	23·5

If the mean be taken of the first and third means, the number 24·35 is obtained, which is double as much as when the bridge was removed. A few other experiments, which it is superfluous to publish here, showed that the amount of diminution in the deflection of the magnetic needle which the induction-currents produce was, by far, not proportional to the number of turns of the induction-spiral, but increased much more slowly.

XX. *On some Phenomena of Binocular Vision.* *By* Joseph LeConte, *Professor of Chemistry and Geology in the University of South Carolina**.

[Continued from vol. xxxvii. p. 140.]

II. *Rotation of the Eye on the Optic Axis.*

NEARLY all the experiments described in this paper had already been made and the results obtained, when my attention was called to Helmholtz's Croonian Lecture "On the Normal Motions of the Eye in relation to Binocular Vision"†. From this lecture I received some useful hints as to the best method of experimenting on this subject, which have been of great service to me, and have made my results much more satisfactory, without, however, materially modifying them. As these results differ very greatly and fundamentally from those of Helmholtz, I repeated the experiments daily for many weeks, modifying them in every conceivable way to avoid the possibility of error. I am perfectly sure, therefore, that the results are true for my own eyes; and as far as I have been able to have them verified, they are true also for most other normal eyes. Unfortunately, however, the difficulty of verification for other eyes is very great. Many of these experiments, which I find perfectly easy, are almost impossible for most persons.

Helmholtz's lecture, I suppose, is the most authoritative statement which we have of the present condition of science on the subjects of the motions of the eye and of the horopter. It seems to be an abstract of more extended researches which I have not seen. On this account it is obscure in some parts; yet I think I cannot be mistaken in his general results. In order to make myself clear, whether in discussing Helmholtz's results or in describing my own experiments, I find it necessary to define the terms I shall most frequently use. The position of the eye when the optic axes are parallel and at right angles to the vertical line of the face, as when with head erect we look at a point on a distant horizon, is called by Helmholtz the *primary direction of the eye*, and the visual line in this case is the *primary direction of the visual line.* All other directions are called *secondary directions*. A plane which passes through the visual line is called a *meridian plane of the eye*, and the intersection of such a plane with the retina we will call a *meridian of the eye*. The *vertical line of demarcatian* is that meridian of the eye upon which the image of an apparently vertical line falls when we look directly at the line, and which therefore divides the retina into two equal halves containing corresponding points

* From Silliman's American Journal for March 1869.

† Proc. Roy. Soc. April 1864, vol. xiii. p. 186.

in the two eyes. The *horizontal line of demarcation* is that meridian of the eye upon which, under similar circumstances, the image of an apparently horizontal line falls. The plane which passes through the two visual lines we will call the *visual plane*, and that visual plane which is at right angles to the line of the face the *primary visual plane.* The line joining the root of the nose and the point of sight, and which therefore bisects the angle of optic convergence, we will call the *median line of sight.*

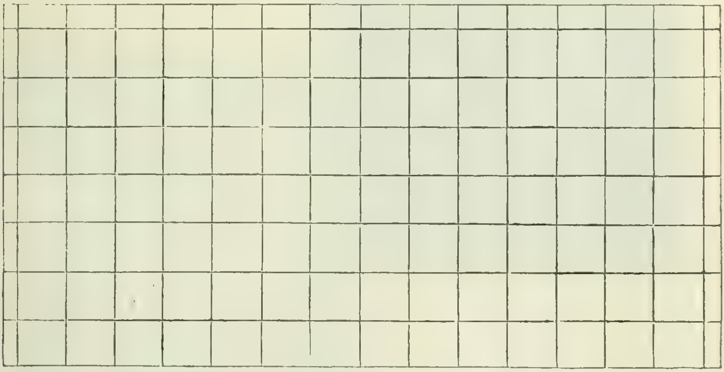
Now Helmholtz gives as the law controlling all the movements of the eye the following, viz. that when the eye turns from its primary to any secondary position, *it turns "on a fixed axis which is normal both to the primary and to the secondary visual line."* In other words, the eye may turn on any axis at right angles to the optic axis, but *does not rotate about the optic axis.* Again, he states that "vertical and horizontal lines keep their vertical or horizontal position in the field of vision when the eye is moved from its primary direction vertically or horizontally." This law had been previously stated by Listing, but without proof; Helmholtz claims to have established it by experiment. His method is very ingenious. It is well known that if we look for some time at a bright object, and then turn the eye upon a comparatively obscure field, a spectrum having the form of the object will be seen. As such spectra are the result of a temporary modification of the retina itself, they must follow the motions of the eye with the greatest exactness. If therefore the bright object be a *line,* then if there be any rotation of the eye on the optic axis, in turning the eye in various directions the linear spectrum ought to incline to one side or the other. Suppose, then, the object be a bright-red vertical line on a grey wall at the exact height of the eye: Helmholtz finds that on gazing at the bright line with one eye, taking care that the eye shall have its primary direction, and then turning the eye in a horizontal plane to the right or left, *the spectrum retains perfectly its verticality.* "I found," he says, "the results of these experiments in complete agreement with the law of Listing." For the ingenious device of Helmholtz for getting the primary position of the eye we must refer the reader to his lecture. I have tried Helmholtz's experiments with similar results. Nevertheless I believe it may be demonstrated that though rotation of the eye does not take place under the circumstances of these experiments, yet it does so under other circumstances not touched by them, and that in a manner which deeply affects the question of the horopter. The law of Listing is doubtless true, or nearly true, when the eyes move together parallel to each other, but is far from being true in strong convergence. The experiments which follow prove beyond a doubt that in my own

case, and in most other cases tried, *the eyes in convergence rotate on the optic axes outward, and that the amount of rotation increases with the degree of convergence.* Meissner* has attempted to determine experimentally the position of the horopter, and from the position thus determined he *infers* the rotation of the eyes: my experiments prove *directly* the rotation of the eyes; and from this, as well as from direct experiment, I hope to establish the position of the horopter.

Helmholtz, it is true, admits some degree of rotation of the eye on the optic axis, particularly when the eye makes wide excursions in the field of view; but that he does not regard this as sufficient to interfere seriously with the law of Listing is evident from the form of the horopter which he deduces. Moreover, according to Helmholtz, these slight rotations are controlled by the law of Donders, viz. "*that the eye returns always into the same position when the visual line is brought into the same direction.*" He regards this law as rigorously exact. "Every position of the visual line," he says, "is connected with a determined and constant degree of rotation." But the experiments about to be described prove that under certain circumstances the law of Donders, too, is far from being true.

We have already stated (p. 136) that when the squares of the ruled diagram (fig. 5) are combined by converging the optic

Fig. 5.



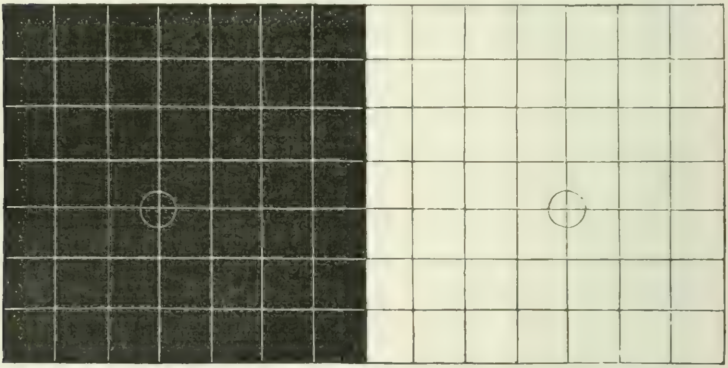
axes, if the amount of convergence be great, the horizontal lines of the two images are distinctly observed to cross each other at a small angle. After my attention was once directed to this fact, I could see slight crossing of the horizontals for every degree of convergence; but the verticals seemed to coalesce perfectly. By placing, however, both the diagram and the head perfectly

* *Bib. Un. Archiv. des Scien.* II. vol. iii. p. 160.

perpendicular, looking straight forward at a point exactly at the same height as the eyes, the visual plane therefore in the primary position, and then slowly increasing or decreasing the convergence of the optic axes so that the vertical lines of the two images *passed slowly over one another*, it was plainly seen that the verticals of the two images were not parallel, but crossed each other at a small angle.

This, my original diagram, however, is not well adapted to experiments on this subject, for two reasons: (1) it is difficult to distinguish the image of one eye from that of the other; (2) it is difficult to control perfectly the convergence of the eyes. When the vertical lines approach each other, they, as it were, leap and cling together as a single line, even though they really cross at a considerable angle; the really crossing lines, by a well-known law of stereoscopic combination, being seen as a single line inclined to the visual plane. I therefore constructed a similar diagram, one-half of which consisted of black lines on a white ground, and the other half of white lines on a black ground. It is convenient also to have two small circles, one on each half and similarly situated (fig. 6). If I place such a diagram perfectly

Fig. 6.

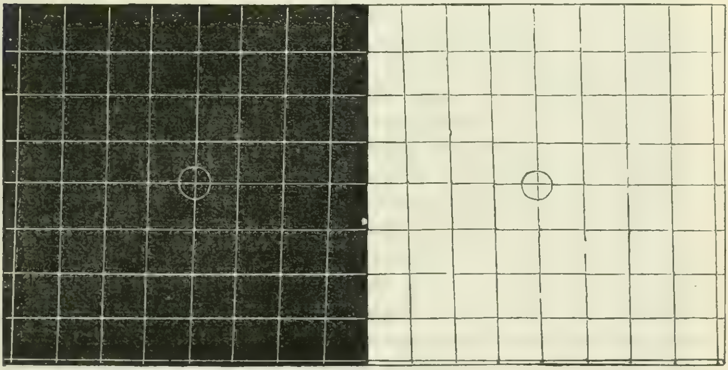


perpendicularly before me, with the head perfectly erect and the eyes at precisely the same height as the small circles, and then stereoscopically combine the circles by crossing the eyes, I distinctly see the white and black lines, both vertical and horizontal, crossing one another at small angle, as if the images of both eyes had rotated on the visual line in opposite directions. This angle of crossing increases as the plane of the diagram is brought nearer, and decreases as the diagram is carried further from the eyes. Or these different angles of crossing may be obtained without moving the diagram or the head, by converging the eyes more and more and causing the white

and black vertical lines to pass successively over each other. This is more easily done if there are several small circles on each half, similarly situated but at different distances from each other. In this diagram, the lines being of different colours do not stereoscopically combine easily—they do not cling together as in the other case. Their approach toward, or recession from, one another, and the angle which they make with one another, may be marked with the utmost exactness. Nor is there any danger of confounding the two images; for since the eyes are crossed, we know that the *white lines* belong to the *right eye* and the *black lines* to the *left eye*; we can therefore determine the direction in which each image rotates. I find always that the black lines or the *image of the left* eye rotates to the right, and the white lines or the *image of the right* eye rotates to the left. Now, as the image always moves in a direction contrary to the motion of the eye (differing in this respect from spectra), this indicates *a rotation of both eyes on the optic axes outward*.

To test this question still further, I constructed another diagram, with the horizontal lines continuous across, but the verticals not perfectly vertical, the upper ends of those of the right half inclining to the left, and those of the left half to the right, by about 1° 20′ (fig. 7). On bringing the circles together I found that at a certain distance of the diagram (but only at a certain

Fig. 7.



distance, depending upon the interval between the circles) the *verticals coalesced perfectly*; the horizontals, however, as might have been expected, still crossed at a small angle, and in the same direction as before; viz. the whites or right-eye image thus, and the blacks or left-eye image thus, indicating in this case also rotation of each eye outward. *Beyond* the proper dis-

tance the verticals approach but do not attain parallelism; *within* the proper distance they cross in a direction contrary to that in the diagram. When the circles are ten inches apart, the proper distance is nearly three feet, and the image therefore about seven inches from the eyes.

Helmholtz has a diagram similar in all respects to my own, except turned upside down, in which, he states, both verticals and horizontals coincide perfectly when the circles are combined. Our own figure (fig. 7) turned upside down will answer for Professor Helmholtz's. We quote his own words:—"The horizontal lines are parts of the same straight line; the vertical lines are not perfectly vertical. The upper end of those of the right figure are inclined to the *right*, and those of the left figure to the left, by about $1\frac{1}{4}°$." But his experience differs from our own in a most unaccountable manner. He says: "Now combine the two sides stereoscopically, *either by squinting or by a stereoscope*, and you will see that the white lines of the one coincide with the black lines of the other as soon as the centres of both figures coincide, although the vertical lines of the two figures are not parallel to each other." He accounts for this, not by rotation of the eyes, but by *the principle of the difference between real and apparent verticality*. The ignorance of this principle he believes has vitiated the results of all previous observers. He illustrates this principle thus: "When you draw on paper a horizontal line, and another line crossing it exactly at right angles, the right superior angle will appear to your right eye too great and to your left eye too small; the other angles show corresponding deviations. To have an apparently right angle, you must make the vertical line incline by an angle of about $1\frac{1}{4}°$ for it to appear really vertical. We must distinguish, therefore, between the *really* vertical lines and the *apparently* vertical lines in the field of view. . . . . Now look alternately with the right and the left eye at these figures (fig. 7 turned upside down). You will find that the angles of the right figure appear to the right eye equal to right angles, and those of the left figure so appear to the left eye; but the angles of the left figure appear to the right eye to deviate much from a right angle, as also do those of the right figure to the left eye." Professor Helmholtz therefore believes that the perfect stereoscopic coincidence of the vertical lines of his diagram is the result of this principle. "Therefore," he says, "not the really vertical meridians of the two fields correspond as has been hitherto supposed, but the apparently vertical meridians. On the contrary, the horizontal meridians really correspond, at least for normal eyes which are not fatigued."

On this principle Professor Helmholtz builds his whole theory of the horopter. But that this principle cannot account for the

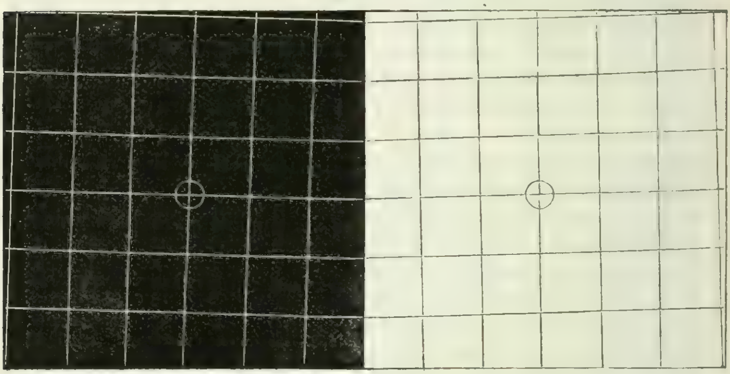
phenomena he observes, I think can be proved. In the first place, I find that if there be any distinction between real and apparent verticality for my eyes, the difference is too small to be detected by the simple observation of lines drawn at right angles with each other. For my own eyes really vertical lines are also apparently vertical, and lines inclined $1\frac{1}{4}°$ from verticality are not at all apparently vertical. I have tried several other normal eyes with the same result. But, leaving this aside, in the second place, it is by no means indifferent whether the two halves be combined by a "*stereoscope or by squinting.*" If they are combined by a stereoscope as stereoscopes are usually constructed, the right half is looked at by the right eye and the left half by the left eye, so that the point of sight and the plane of combination is *beyond the diagram*; coincidence in this case, therefore, would be a true illustration of Professor Helmholtz's principle. But if they are combined by squinting, the eyes are crossed, and therefore *the right eye is looking at the left half and the left eye at the right half* of the diagram, and therefore, in Professor Helmholtz's own words, the verticals should "deviate much from a right angle," viz. $2\frac{1}{2}°$. I have tried many eyes and I have yet found *none* in which the coincidence of the verticals of Professor Helmholtz's diagram was perfect when combined by means of a stereoscope, *i. e.* beyond the diagram; but I have found *one* person to whom the coincidence seemed to be perfect when the combination was made by squinting.

It is evident, then, that Professor Helmholtz's principle cannot explain the stereoscopic coincidence by squinting in his own experiment. I myself believe that if the coincidence takes place only by squinting (as in the case mentioned above), it can only be explained by rotation of the eyes *inward.* It is true that in this case the horizontals ought to cross also; but Professor Helmholtz himself admits that such is sometimes the fact, but attributes it to fatigue. "After keeping the eyes," he says, "a long time looking at a near object, as in reading or writing, I have found that the horizontal lines cross each other; but they became parallel again when I had looked for some time at a distant object."

On reading Professor Helmholtz's lecture and finding his results so different from my own, I immediately tried his figure by squinting, but found the verticals cross one another at an inclination much greater than in the diagram itself, while the horizontals also crossed but at a less angle. On turning the figure upside down, however, the verticals coincided perfectly when the proper distance was obtained, though the horizontals crossed as before. All these phenomena are easily explained by rotation of the eyes *outward.* To test the question still more thoroughly, I then constructed other diagrams in which both verticals and

horizontals were inclined so as to make an angle of $1\frac{1}{4}°$ with the true vertical and the true horizontal (fig. 8), and therefore perfect squares with one another. At the proper distance, when the small circles were brought together, *the coincidence of both verticals and horizontals seemed to be perfect.* When the plane of the diagram was too near or too far, all the lines crossed, in the one case in one direction and in the other case in the other direction. I then constructed still other diagrams, in which the inclination of the lines with the true vertical and the true horizontal were 40 minutes, $2\frac{1}{2}$ degrees, and 5 degrees. In all cases I brought the lines into coincidence, but of course by different degrees of convergence. In the last the optic convergence necessary was extreme, and the strain on the eyes considerable; but in the other cases there was not the slightest difficulty or strain. Recollecting, however, that Professor Helmholtz supposed that the change of position of the horizontals might be the result of fatigue, I tried repeatedly after long rest, but always the phenomena were precisely the same. In the diagram in which the inclination of the lines was 5 degrees I observed, however, that a *greater degree of*

Fig. 8.



convergence was necessary to bring the horizontals into coincidence than to bring the verticals into coincidence. The difference in the distance of the diagram in the two cases was about two inches, and the difference in the distance of the point of sight was about half an inch. I cannot explain this except by supposing that the form of the optic globe was changed by the excessive action of the muscles.

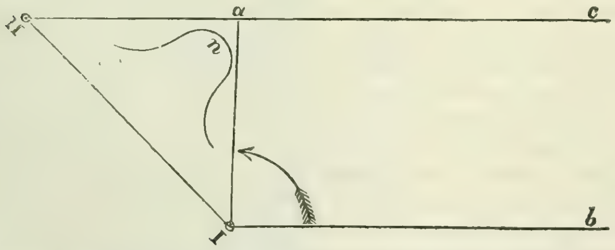
I can conceive of no possible source of fallacy in these experiments. From long practice they have become almost as easy to me as any ordinary act of vision. They do not now fatigue my eyes in the slightest degree. I see the lines of the two images,

which I bring together just as plainly as if they were black and white threads. While watching them I control their motions almost as perfectly as if I was sliding with my hands two frames with white and black threads stretched across them. There is not the shadow of a doubt, therefore, that in my own case the eyes in convergence rotate slightly outward, and that the amount of rotation increases with the degree of convergence.

I next proceeded to determine the amount of rotation for different distances of the point of sight. In the diagram in which the inclination of the lines was 5 degrees, the distance of the image was only 2 to 2½ inches; for the lines inclined 2½ degrees, the distance of the image was 4 inches; for lines inclined 1¼ degree the distance was 7 inches; and for 40 minutes the distance was about 12 to 14 inches. I am able by great strain to obliterate, or nearly obliterate, the common field of view of the two eyes. In this case, of course, the eyes both look at the root of the nose. In this extreme convergence I find that lines coincide which make with each other an angle of 22°, or 11° with the vertical. This would seem, therefore, the extreme rotation for my eyes. The distance of the image in this case is nearly at the root of the nose.

If, however, in extreme convergence rotation on the optic axes takes place to the extent of 11°, this rotation ought to be detectable by means of ocular spectra, or even by direct observation of the eye itself. I determined to try these also. My method of experimenting with ocular spectra is as follows:—Standing in a somewhat obscure room, I gaze with the left eye (the other being shut) at a vertical crevice in a closed window until a distinct spectrum is obtained. Placing myself now opposite a vertical line on the wall of the room, with my right side toward the wall, I turn my head until my left eye II (fig. 9), look-

Fig. 9.



ing across the root of my nose, *n*, can see the vertical line. I now gaze at a point very near the vertical line, and, by inclining my head to one side or the other, bring the spectrum exactly parallel to the vertical line. In this position, if the wall be at

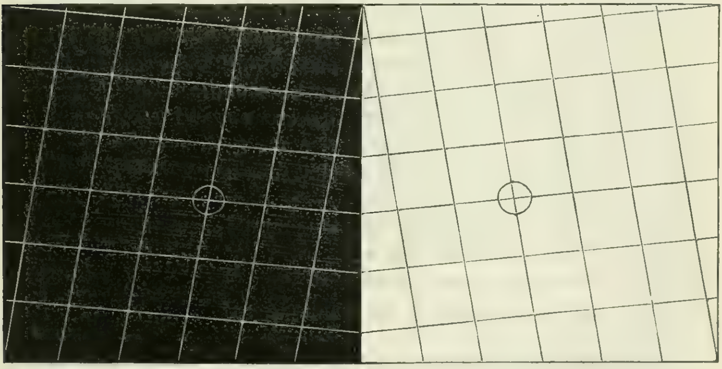
some distance from the observer, the axes of the eyes may be regarded as nearly parallel as II *c*, I *b*. I now by a voluntary effort bring the point of sight along the line II *c* nearer and nearer, until it reaches *a* near the root of the nose. In doing so the spectrum is always seen to incline to the left, thus \. On relaxing the convergence and looking again at the wall, the spectrum retains its inclined position for an appreciable time and then gradually recovers its original verticality. In similar experiments with the right eye the spectrum is always seen to incline to the right, thus /.

I next tried direct observation of the eye itself. As I could not find ony one with the necessary control over the eyes, I was compelled to make myself the subject of this observation. While, therefore, with the right eye shut I gaze with the left eye across the root of the nose on vacancy, or on a distant object as in the figure (fig. 9), an observer, conveniently placed near the visual line, carefully examines the iris of my eye so as to recognize the position of the radiating lines. When now, without changing the position of the visual line of the left eye, I turn the right eye inward as in the previous experiment, until the point of sight is at *a*, the globe of the left eye is distinctly seen to rotate outward. I got four different persons to make this observation upon my eye, and the testimony of all was the same.

I had proceeded thus far in my experiments when I was led to reflect further upon the phenomena presented by the diagram in which the lines were highly inclined. In this diagram, it will be remembered, the verticals were combined with more facility than the horizontals. I now repeated all my experiments with more care and with especial reference to this point. As I expected, I found the same true for all the diagrams; but the difference was so small that it had escaped detection. This led me to suspect that there might be some truth in Professor Helmholtz's principle of real and apparent vertical. I therefore constructed many other diagrams to test this point. I constructed first a diagram exactly like fig. 6, except that the circles were the same distance apart as my eyes, viz. $2\frac{1}{2}$ inches. On placing this diagram before me and gazing on vacancy, the eyes therefore in their primary position, the circles were brought together. In this experiment the *verticals came together parallel*. I sometimes thought there was a scarcely perceptible inclination in the direction required by Helmholtz's principle, viz. thus /\. If any such inclination really existed, it could not have been more than

10′ for each line with the vertical, or 20′ with one another; for this angle I can distinctly detect under these circumstances. I next constructed a diagram like Professor Helmholtz's, except that the outward inclination of the verticals was only 40′ instead of $1\frac{1}{4}$°. On combining the two halves of this diagram by means of a stereoscope, there really seemed to be perfect coincidence of both verticals and horizontals; but I soon found, by trying several, that stereoscopes differ much in this respect. I therefore discarded them as unreliable. On combining the same diagram with the naked eye in the manner of a stereoscope, *i. e.* beyond the plane of the diagram, the verticals coincided perfectly when the point of sight was about twelve inches distant, but the horizontals very perceptibly crossed, though certainly, I think, at an angle less than 40′ (it seemed about 20′). On combining the two halves by squinting (of course turning the diagram upside down), I found the result precisely the same when the point of sight was at the same distance, viz. 12 inches. In the next diagram which I constructed the verticals inclined $1\frac{1}{4}$° and the horizontals 50′, the difference being therefore 25′. In this case both seemed to combine perfectly when the point of sight was distant $7\frac{1}{2}$ inches. The next diagram tried had the verticals inclined 5° and the horizontals 3° 45′, the difference being $1\frac{1}{4}$°. In this case both verticals and horizontals combined perfectly at the distance of 2·2 inches. I then tried one in which the verticals inclined 10°. In this case I could not make perfect coincidence of both verticals and horizontals until the difference of inclination was made as great as 5°. The diagram used is shown reduced in the figure (fig. 10). The point of

Fig. 10.



sight in this experiment was only $1\frac{1}{4}$ inch from the line joining

the optic centres, or about a quarter of an inch from the root of the nose.

I attribute these phenomena to a slight distortion of the ocular globe under the action of the oblique muscles—a distortion which increases with the degree of optic convergence. We will refer to this again.

In all the experiments described above, the greatest care was taken that the visual plane should be in the primary direction, *i. e.* at right angles to the line of the face, and especially that the median line of sight should be at right angles to the plane of the diagram. I now wished to try the effect of turning the visual plane upward and downward. Meissner, from his experiments on the position of the horopter, had arrived at the conclusion that the rotation of the eye was zero, whatever the degree of convergence, when the visual plane was inclined downward 45° from its primary position, and that the rotation increased as the plane was elevated toward the eyebrows. I was anxious to test this result. The plane of the diagram still remaining vertical, I now turned the face upward (taking care, however, that the eyes should still be on an exact level with the circles of the diagram) until the eyes looked in the direction of the point of the nose. In this position, on stereoscopically combining the small circles, the lines, both vertical and horizontal, in all cases *maintained their true position*: i. e. in the diagram with parallel lines (fig. 6), the coincidence of all the lines was perfect; in the diagram with inclined verticals (fig. 7), the horizontals coalesced perfectly and the verticals crossed at their true angle of inclination; while in the diagram with the verticals and horizontals both inclined (fig. 8), both the verticals and horizontals crossed at their true angle of inclination. I tried the same experiment for various distances, and therefore various degrees of optic convergence, but always with the same result. *There is,* therefore, *no rotation* of my eyes *when the plane of vision is inclined* 45° *downward.* In continuing the inclination still further downward, I observed a decided rotation of the eyes in the contrary direction, i. e. *inward.* As the eyes are raised from the position 45° downward, the rotation increases until the visual plane is again in its primary direction. When the visual plane is raised above this, however, I do not find the rotation to increase as stated by Meissner, except in cases of extreme convergence, but rather to decrease again, although it does not again become zero*. In

* More recent experiments, just concluded, have convinced me that in my own eyes, if the convergence is very slight, the outward rotation *does* reach zero and may even be converted into an *inward* rotation. The reason

strong convergence, however (as, for instance, when the point of sight is less than seven inches distant), the rotation continues to increase as stated by Meissner.

In all these experiments, in order to detect the true rotation, it is absolutely necessary that the median line of sight should be exactly at right angles with the plane of the diagram. The least error in this respect will cause *perspective convergence* of the parallel verticals, or increase or decrease of the angle of inclination of the inclined verticals. With the diagram three feet distant, if my eyes look one inch above or below their true level, on combining the two halves of the diagram I can detect the perspective convergence, upward or downward, with the greatest ease. In all cases also, but particularly when the convergence is very strong, it is necessary to fix the attention on that horizontal which passes through the small circle; for those above and below converge by perspective.

In these experiments the size of the diagrams is of little importance. I have used them of every size from 5 by 10 inches to 15 by 30 inches.

My next desire was to determine how far these results were general for normal eyes. The great difficulty was to find any one who was able to repeat the experiments. Nevertheless I have found four young persons with normal eyes who, with some practice, have succeeded in all except the most difficult of them. *Their results agreed perfectly with my own.* In a fifth case, however, in which great difficulty was experienced and the results were uncertain, I was led to believe that the eyes in convergence rotated *inward.* It is not improbable, therefore, that normal eyes differ in this respect.

We believe, therefore, that we are justified in the conclusion that when the eye is in its primary position and therefore passive, the vertical line of demarcation coincides with the vertical meridian, and the horizontal line of demarcation with the horizontal meridian of the eye, and therefore these two lines of demarcation are at right angles to each other. But as soon as the eyes begin to converge, the oblique muscles (particularly the inferior oblique) begin to act, rotating the eye on the optic axis and slightly distorting its form; so that the vertical line of demarcation is now not only no longer coincident with the vertical meridian, but also no longer at right angles to the horizontal

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is, that when my eyes are parallel or nearly so, elevation of the visual plane causes *inward* rotation. In some other eyes, however, I have found that elevation of the visual plane when the eyes are parallel causes outward rotation as stated by Meissner. In these cases, therefore, Meissner's results on this point are entirely true.

line of demarcation. Both the rotation and the change in the relation of the two lines of demarcation increases with the degree of optic convergence. It is possible that the frequent action of the muscles distorting the globe of the eye may leave some permanent impress upon the form of the globe, so that even in a passive state the vertical line of demarcation does not coincide perfectly with the vertical meridian. If so, then to that extent Helmholtz's principle of real and apparent vertical in the primary position of the eye will be true. Or, to express it differently, we have seen that the inclination of the vertical upon the horizontal line of demarcation decreases as the point of sight recedes; at $1\frac{1}{4}$ inch it is $5°$, at 2·2 inches it is $1\frac{1}{4}°$, at 7·5 inches it is $25'$, and at 12 inches $20'$. It is possible that even when the point of sight recedes to infinite distance, and the horizontal line of demarcation becomes coincident with the horizontal meridian, the vertical line of demarcation may still make a small angle with the vertical meridian. If so, this angle is the difference between the real and apparent vertical spoken of by Professor Helmholtz. We do not yet admit this as probable, however; for the natural position in which all lines at all distances combine when the visual plane is inclined $45°$ downward seems inconsistent with this idea.

The decrease of the rotation of the eye when the visual plane is inclined downward, and its increase when the visual plane is inclined upward, seem to be the result of the relative power of the two oblique muscles. Ordinarily the inferior oblique is the stronger, and the rotation is therefore outward; but as the visual plane is inclined downward, the action of the two become more and more nearly equal, until at $45°$ they balance each other and there is no rotation. Below $45°$ the action of the superior oblique predominates, and the eye therefore rotates inward. In turning the visual plane upward and converging strongly, the action of the inferior oblique predominates more and more.

It will be observed that the rotation of the eye which we have demonstrated necessitates, in optic convergence, a difference between the real and apparent vertical; but our views differ entirely from those of Professor Helmholtz in the following respects:—(1) Professor Helmholtz admits only a difference between real and apparent *vertical*; we have shown a difference between the real and apparent horizontal as well as the real and apparent vertical. (2) Professor Helmholtz's difference is a constant one, viz. $1\frac{1}{4}°$; ours varies from $11°$ to $20'$, and probably to zero. (3) According to Professor Helmholtz, the relation of the apparent vertical to the apparent horizontal is a constant one, viz. an angle of about $88\frac{3}{4}°$; our experiments prove that this relation varies to the extent of $5°$.

It is certain, therefore, that the law of Listing is far from being true in strong convergence. Evidently the reason is, that in convergence muscles are used which are not used in simply turning the eyes from side to side, as in the experiments used by Helmholtz to prove this law (p. 180). That different muscles are used in strong convergence is easily shown as follows :—It is easy to turn either eye inward until it looks in the direction of the root of the nose, provided the other eye moves parallel with it, *i. e.* outward; but it is almost impossible to turn both eyes at the same time so as to look at this point. Great strain is experienced in producing convergence even much short of this. The eyes are turned from side to side, parallel to each other, by means of the interior and exterior recti muscles, while in convergence the oblique muscles are also used. For this reason Professor Helmholtz's experiments on spectra do not apply to convergence.

The law of Donders is equally untrue for strong convergence. This law asserts that the position of the eye is rigorously constant for every position of the visual line. But in the experiment represented by fig. 9, the eye II, *although the direction of its visual line is unchanged, rotates on its axis* when the visual line of the other eye is turned from the direction I *b* to the direction I *a*.

The reason is, that as I turns toward *a* the oblique muscles in both eyes begin to act. It is probable that the action of the oblique muscles, and therefore the rotation of the eye, is *consensual with the two adjustments and with the contraction of the pupil*; and it is well known that, under the circumstances represented by the figure, the pupil of the eye II would contract also, although the direction of the visual line is unchanged.

III. *The Horopter.*

If we look intently at any point, the visual lines converge and meet at that point. Its image is therefore impressed on exactly corresponding points of the two retinæ, viz. on the central spot of each. A small object at this point is therefore seen single. We have called this point the *point of sight.* All objects beyond or on this side of the point of sight are seen double, for their images do not fall on corresponding points of the two retinæ. But objects above or below, or to one side or the other of the point of sight, may possibly be seen single also. *The sum of all the points which are seen single,* while the point of sight remains unchanged, *is called the horopter.* Or it may be expressed differently thus: each eye projects its retinal images outward into space, and therefore has its own field of view crowded with its

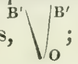
own images. When we look at any object, we bring the two external images of that object into coincidence at the point of sight. Now the point of sight, together with all other corresponding points of the two fields of view which coalesce at that moment, constitute the horopter. Of course the images of all points lying in the horopter fall on *corresponding points* of the retina.

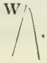
Is the horopter a surface or is it a line? In either case what is its form and position? These questions have tasked the ingenuity of physicists, mathematicians, and physiologists. If the position of identical points of the retinæ under all circumstances were known, then the question of the form of the horopter would become a purely mathematical one. But the position of identical points evidently depends upon the laws of ocular motion. It is evident, therefore, that it is only on an experimental basis that a true theory of the horopter can be constructed; and yet the experimental investigation as usually conducted is very unsatisfactory, on account of the indistinctness of vision when the object is at any considerable distance from the point of sight in any direction.

The most diverse views have, therefore, been held as to the nature and form of the horopter. Aguilonius, the inventor of the name, believed it to be a plane passing through the point of sight and perpendicular to the median line of sight. Others have believed it to be the surface of a sphere passing through the point of sight and the optic centres; others, a torus formed by the revolution of a circle passing through the point of sight and the optic centres on a line joining the optic centres. The subject has been investigated with great acuteness by P. Prévost, A. Prévost, J. Müller, G. Meissner, E. Claparède*, and, lastly, by Helmholtz†. A. Prévost determines in it, as he supposes, a circle passing through the optic centres and the point of sight, which he calls the "*horopteric circle*," and a straight line passing through the point of sight at right angles to the visual plane, which he calls the "*horopteric vertical*."

Until the investigations of Meissner, almost all attempts to determine the form of the horopter have been by mathematical calculations, based upon the doctrine of identical points, and assuming the law of Listing. Meissner attempts the same question experimentally. We condense the following account of his admirable investigations from Claparède's memoir on this subject‡ already referred to.

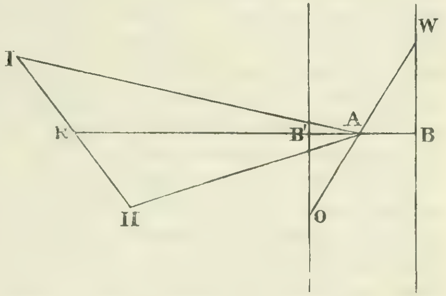
* *Bib. Un. Archiv. des Scien.* II. vol. iii. pp. 138 & 225.
† Proc. Roy. Soc. April 1864.
‡ *Bib. Un. Arch. des Scien.* II. vol. iii. p. 138.

Let R (fig. 11) be an observer and I, II his two eyes, A the point of sight, B an object beyond and B′ an object nearer than the point of sight, but all in the same line, joining the root of the nose and the point of sight. Of course both B and B′ will be seen double. If, now, while the sight is still fixed upon A, B be elevated, its two images, according to Meissner, will approach until at some point, W, they coalesce. If, on the contrary, B be depressed, its images separate more and more. If, now, B′ be elevated, its images separate; but if it be depressed, its images approach and coalesce at O. The line W A O is, therefore, the horopter or line of single vision. It is not at right angles, but inclined to the plane of vision. Again, according to Meissner, if instead of points we have vertical lines like threads, W B and O B′ (fig. 11), then O B′ will double at B′, the images being crossed, and they will approach one another and meet at O, in other words, will appear thus, 

; while B W will also double at B but not cross (*i. e.* each image will have the same name as the eye), and will be seen to converge and meet at W thus, 

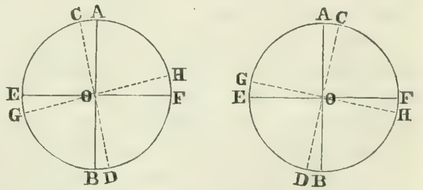
. Lastly, if the vertical line pass through the point of sight A, the images will cross one another like an **X**.

Fig. 11.



Meissner accounts for these phenomena by supposing that, in converging the optic axis, the eyes rotate on the optic axis outward, so that the vertical lines of demarcation C D (fig. 12) no longer coincide perfectly with the vertical meridians A B, as they do when the eyes are in the primary direction (the axis parallel), but cross them at a small angle. In the primary direction of the eye the image of a vertical line, according to Meissner, falls on the vertical line of demarcation C D in both eyes (for these lines then coincide

Fig. 12.



with the vertical meridian) and is therefore seen single. But if the eyes rotate on the optic axes outward, then the image of a vertical line still falling on the vertical meridian must cross the line of demarcation in opposite directions in the two eyes, and therefore cannot be seen single except at the point of sight, the image of which corresponds to the central point O of the retina of each eye. In order that the image of a line shall fall on the line of demarcation in both eyes and thus be seen single, it must be inclined at a certain angle with the vertical, the lower end being nearer and the upper end further away. It is moreover evident, upon a little reflection, that when the eye rotates, the horopter cannot be a plane or a surface of any kind; for objects right and left of the horopteric line must all be doubled by displacement of the horizontal line of demarcation G H (fig. 12), which therefore no longer coincides with the horizontal meridian, E F.

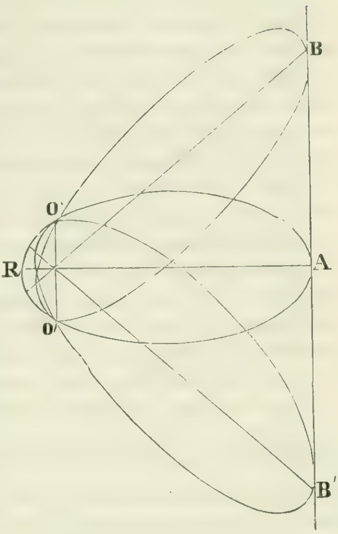
From various experiments made at different distances and with different degrees of inclination of the visual plane upward and downward, Meissner concludes:—(1) That, looking straight forward at an infinite distance, the horopter is a plane at right angles to the visual lines. (2) That for all other distances, the visual plane remaining the same, the horopter is a straight line passing through the point of sight and increasing in inclination to the visual plane as the convergence of the optic axes increases. (3) That in turning the visual plane downward, the inclination of the horopteric line with that plane becomes less and less, until at 45° downward it becomes perpendicular, and therefore the horopter again expands into a plane at right angles to the median line of sight. (4) That in raising the visual plane upward toward the eyebrows, the inclination of the horopter to the visual plane increases.

We have given Meissner's investigations more in detail, because by entirely different methods we have confirmed almost all of them.

Claparède by similar experiments fails to confirm the conclusions of Meissner, and therefore rejects them. He concludes, partly from his own experiments and partly from calculation, that "the horopter is a surface of such a form that it contains a straight line perpendicular to the plane of vision and passing through the point of sight, and that every plane passing through the optic centres makes, by intersection of this surface, the circumference of a circle." In other words, he believes that the horopter is a surface which contains the horopteric vertical B A B′ (fig. 13) and the horopteric circle O A O of Prévost, and that in addition the surface is further characterized by the fact that, while the point of sight remains at A, the intersection

with it of every plane passing through the optic centres O, O′ upward or downward as O B O′ and O B′ O′ is also a circle. It is evident that as these circles would increase in size upward and downward, the horopter, according to Claparède, must be a surface of singular and complex form.

Fig. 13.



Finally, Helmholtz arrives at results entirely different from those of all previous observers. He sums up his conclusions as follows:—

"When the point of convergence is situated in the middle [vertical] plane of the head, the horopter is composed of a straight line drawn through the point of convergence [direction not stated, but evidently not at right angles to the visual plane, for see below the sentence marked [a]], and a conic section passing through the optic centres and intersecting the straight line."

"When the point of convergence is in the plane which contains the primary visual lines [primary visual plane], the horopter is a circle going through that point and the optic centres [Prévost's horopteric circle] and a straight line intersecting the circle [where and in what direction not stated]."

"When the point of convergence is situated as well in the middle plane of the head as in the primary visual plane, the horopter is the circle just described [Prévost's horopteric circle] and a straight line going through that point [direction not stated]."

"There is but one case in which the horopter is really a plane, viz. when the point of convergence is in the middle plane of the head and at an infinite distance. Then the horopter is a plane *parallel* to the visual plane and *beneath* it, at a certain distance which depends upon the angle between the *really* and *apparently vertical* meridians, but which is nearly as great as the distance of the feet of the observer from his eyes when he is standing. Therefore, when we look at a point on the horizon, the *horopter is the ground on which we stand.* [a]When we look at the ground on which we stand at any point equally distant from both eyes, the horopter is not a plane; but the straight line which is one of

its parts *coincides completely with the horizontal plane on which we stand.*"

These conclusions of Helmholtz are the result of refined mathematical calculations *based entirely upon the supposed constant difference between the real and apparent vertical.* If this principle be true for all normal eyes, then it is probable that Helmholtz's conclusions in regard to the form and position of the horopter are also true for those cases in which the point of sight is at a considerable distance, and in which, therefore, the rotation of the eye is very small. I am not able to test all of Professor Helmholtz's conclusions by calculations based upon this principle, but I easily see that the position of the horopter lying along the ground is the necessary consequence of a difference of $1\frac{1}{4}°$ between the real and apparent vertical when the eyes are in their primary direction. For if a line be drawn from each pupil downward, making an angle of $2\frac{1}{2}°$ with each other or of $1\frac{1}{4}°$ with the vertical, they will intersect each other at the distance of about five feet below the eyes or about the feet of the observer standing erect. Now if these two lines be placed thus  before the observer whose eyes are in the primary direction, it is plain that their stereoscopic combination would be a line lying along the ground to infinite distance. If the difference between the real and apparent vertical be less than $1\frac{1}{4}°$, then the distance below the eyes of the horopteric plane will be greater. We have already shown that if there be any such difference in our own eyes, it cannot be more than 10′; in this case the horopteric plane will be at least 35 to 40 feet below the eyes. But Professor Helmholtz takes no account of rotation of the eyes on the optic axes, which greatly affects the form and position of the horopter when the point of sight is near; and we believe that it is only when the point of sight is near that the form and position of the horopter is of any practical importance in vision, for it is only then that the doubling of images lying out of the horopter is perceptible.

It has been with much hesitation that I have ventured to criticise the conclusions of so distinguished a physicist. My ability to do so, if well founded, I attribute entirely to a facility in the use of the eyes such as I have never seen equalled in the case of any other person.

Although I believe Meissner has arrived at truer results than any one who has yet written on this subject, yet I think his method very unsatisfactory. I have wondered at the skill and patience which could attain such true results by such imperfect methods. I have tried Meissner's experiments without any satisfactory results, and I confess I commenced these experiments

with the conviction that his theory was untenable; but, contrary to my expectations, his views have been in a great measure confirmed. The difficulty with Meissner's method, and, in fact, with all previous experimental methods, as already stated, is the indistinctness of objects at any considerable distance from the point of sight in any direction. In Meissner's experiment with the three points B′, A, and B (fig. 11), in lowering B′ or elevating B the indistinctness was so great that I could not tell with certainty whether the images approached each other or not; and in his second experiment with the thread, the obstinate disposition on the part of the eye to see single by stereoscopic combination, even when the images cross, interferes seriously with the certainty of the result. But in my experiments, by virtue of the complete dissociation of the axial and focal adjustments, the lines are seen perfectly clearly; and by making them pass each other slowly, their relation to each other may be observed with great exactness.

I will now state my own results in regard to the horopter.

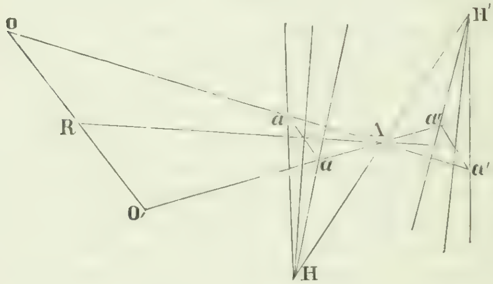
It is evident that if, in convergence, the eyes rotate on the optic axes, as my experiments prove, then in this state of the eyes the horopter cannot be a surface, but a line; and this line cannot be vertical, but inclined to the visual plane. Perhaps this requires further explanation. If the eyes in a state of convergence be fixed on a vertical line, then if the eyes rotate the line must be doubled except at the point of sight. This doubling is the result of *horizontal* displacement of the two images in opposite directions; and therefore the two images may be brought together by bringing the doubled portion of the vertical line nearer or carrying it further away. This is done in inclining the line as in fig. 11. But all points to the right and left of the horopteric line are also doubled by rotation; but this doubling is the result of *vertical displacement* of the images: now vertical displacement cannot be remedied by increasing or decreasing the distance, because *the eyes are separated horizontally.* Therefore no form of surface can satisfy the conditions of single vision right and left of the horopteric line. The restriction of the horopter to a straight line and the inclination of that line to the visual plane are therefore necessary results of rotation on the optic axes. But I have also proved this by direct experiment.

If two lines, one white on black and the other black on white (fig. 14), be drawn at an angle of $1\frac{1}{4}°$ with the vertical, and therefore $2\frac{1}{2}°$ with each other, then by bringing my eyes so near to them at any point *a a* (taking care that the median line of sight shall be perpendicular to the plane of the lines) that the visual lines without crossing shall meet beyond the

diagram at the distance of seven inches from the eyes, the two lines are brought into perfect coincidence. If, on the contrary, the same figure be turned upside down and the eyes be placed a little further than seven inches, so that the two points a, a are brought together by crossing the optic axes at the distance of seven inches, then also the lines are brought into perfect coincidence. The accompanying figure (fig. 15), in which O, O′ are the eyes, A the point of sight, a H, a H, and a' H′, a' H′ are the lines in the two positions, will explain how the stereoscopic combination takes place in each case. The line H′ A H is the horopter. This experiment is difficult to perform satisfactorily. When the lines come together it is difficult to determine whether there is real coincidence or not. I have observed, however, that when the coincidence is not perfect the white and black lines seem to run spirally round each other. The best plan is to observe them at the moment of coming together or of separating. I feel quite confident of the reliability of the conclusions reached.

Fig. 14.

Fig. 15.



I made many calculations, based upon these experiments and on the previous experiments on the rotation of the eye, to determine the inclination of the horopteric line for different degrees of convergence, *i. e.* for different distances of the point of sight. The results of these calculations were not entirely satisfactory. I had expected from Meissner's results that there would be found a progressive increase as the distance decreased. But I could not be sure from my calculations of any increase or decrease with distance. For all distances the inclination seemed to come

out about 7°—in some a little less, in some a little more. Beyond 3 inches there seems to be a slight progressive increase rather than decrease; within 3 inches the action of the eyes was irregular.

I then adopted another method. I used the diagram of parallel lines (fig. 6) and inclined it at an angle of exactly 7° from the perpendicular in the supposed direction of the horopter and at the distance of 3 feet. In this position the verticals, of course, all converge by perspective. I then brought together successively the lines 3 inches apart, then those 6 inches apart, then those 9 inches, 12 inches, 15 inches, 18 inches, and so on even to the last, which were 30 inches apart: *in each case the lines seemed to come together parallel*; or at least the divergence, if any, was so small that I could not be sure about it. Now in this experiment the point of sight varied from $16\frac{1}{2}$ inches to only 2·8 inches in distance, and yet the inclination of the horopteric line seemed to be nearly the same for all, viz. 7°. If there was any difference at all, it seemed to be in favour of *greater inclination at greater distance.* This result (which I arrived at, though doubtfully, by experiment alone) would be the necessary result of any residual difference between the real and apparent vertical, or, in other words, any residual inclination of the vertical upon the horizontal line of demarcation of the eye in its primary position, such as Helmholtz maintains and as I have supposed possible. Still it by no means proves the existence of this residual difference.

It must not be supposed, however, because the lines 3 inches, 6 inches, 9 inches, 12 inches, &c. apart are all brought into coincidence at the same or nearly the same inclination, that therefore the amount of rotation of the eye is the same for all. The perspective convergence of the lines, of course, increases with their distance apart, and therefore the rotation of the eye necessary to bring them successively into coincidence increases also. It is quite possible that the rotation should increase with the optic convergence, and yet the inclination of the horopteric line remain constant or even decrease with the convergence. Whether the inclination of the horopteric line increases or decreases with distance would depend upon the law of increase of rotation with increasing convergence. If it increases with distance, then it is possible that when we look at the ground before us the horopter may be a line lying along the ground, as maintained by Helmholtz.

I next tried the same experiments with the eyes inclined downward 45°. The *lines do not change at all their natural perspective convergence.* In all the experiments made with eyes in this position the inclination of the lines in the image was the

same as in the object. I conclude, therefore, that in this position of the eyes the horopter is at right angles to the plane of vision; and since there is no rotation of the eye, the horopter in this position expands *into a surface*. Below this inclination *the horopter again becomes a line, but inclined now the other way,* i. e. *the upper end towards the observer*. In turning the eyes upward toward the eyebrows, I have found the rotation, except in cases of strong convergence, less than looking straight forward. I conclude, therefore, that in this position the horopteric line inclines less to the visual plane than it does when the visual plane is in its primary direction*.

The points in which my experiments do not confirm Meissner are (1) the increasing inclination of the horopteric line with increasing convergence, (2) the increasing rotation of the eye as well as inclination of the horopteric line under all circumstances in turning the eye upward. Again, I believe that Meissner is also wrong in supposing that the horopter is a *plane* when the eyes are depressed 45°. In this position it is a *surface*, but not a plane. It is clear that the images of points situated to the right and left of the point of sight and in the same plane with it cannot fall on corresponding points of the two retinæ. As to the form of this surface, I feel myself unequal to the task of its mathematical investigation; and its experimental investigation presents, I believe, insuperable difficulties.

We have seen that the eye in convergence rotates on the optic axis. The question naturally occurs, Is this rotation to be regarded in the light of an imperfection of the instrument (of which there are several examples in the structure and mechanism of the eye), and should the law of Listing be regarded as the ideal of ocular motion, though an ideal seldom or never realized in nature? or does the rotation of the eye subserve some useful purpose in vision? I believe there is no doubt that the latter view is the correct one; for there seem to be special muscles which are adapted for this rotation, and the action of these muscles is consensual with the adjustments of the eye and the contraction of the pupil. This purpose I explain as follows. A general view of objects in an extended field is absolutely necessary to animal life in its highest phases, but an equal distinctness of all objects in this field would only distract the attention;

* As stated in note on p. 190, eyes certainly differ in this respect. In my own, if convergence be small, the outward rotation decreases with the elevation of the visual plane, becomes zero, and is even converted into an *inward* rotation; the inclination of the horopter, therefore, decreases, *becomes perpendicular, and even inclines the other way.* In some other eyes the outward rotation increases whatever be the convergence; in this case, of course, the inclination of the horopter increases as stated by Meissner.

therefore the eye is so constructed and moved as to restrict as much as possible both *distinct* vision and *single* vision. Thus as in *monocular vision* the more elaborate structure of the *central spot* of the retina restricts distinct vision to the visual line, and the *focal adjustment* still further restricts it to a single point in that line, so also in *binocular* vision, *axial adjustment* restricts single vision to the horopter, while rotation restricts the horopter to a single line.

Conclusions.

The most important conclusions arrived at in this paper may be briefly summed up as follows:—

(1) The axial and focal adjustments of the eye are not so inseparably associated as is generally supposed; but, on the contrary, when distinctness of vision requires it they may be completely dissociated*.

(2) In this dissociation the contraction of the pupil associates itself with the focal in preference to the axial adjustment.

(3) In optic convergence there is a rotation of both eyes on the optic axes *outward*, and this rotation increases with the degree of convergence.

(4) In inclining the visual plane downward, the rotation of the eyes for the same degree of convergence decreases until, when the visual plane is inclined 45° downward, the rotation becomes zero for all degrees of convergence. Below the inclination of 45° the rotation is *inward*. In turning the eyes upward, except in cases of strong convergence, the rotation also decreases slightly but does not reach zero†; in strong convergence it increases as stated by Meissner.

(5) Besides the rotation produced by optic convergence, there is also a decided inclination of the vertical line of demarcation upon the horizontal line of demarcation, which increases with the degree of convergence. This change in the relation of these two lines is probably the result of distortion of the ocular globe.

(6) As a necessary consequence of the rotation of the eyes, for all degrees of convergence in the primary visual plane the horopter is a *line* inclined to the visual plane, the lower end nearer the observer; but whether the inclination increases or decreases with distance I have not been able to determine with certainty. It probably increases with distance.

(7) In inclining the visual plane below the primary position, the inclination of the horopteric line becomes less and less until,

* While these pages were passing through the press, I discovered that in this conclusion I had been anticipated by Donders and others. All previous experiments, however, were made by means of glasses. Mine were made with the naked eye.

† See this statement modified in note on p. 190.

when the visual line is lowered 45°, the horopteric line becomes perpendicular to that plane and at the same time expands into a surface. Below 45° the horopter again becomes a line, but now inclined in the contrary direction, *i. e.* the *upper* end nearer the observer.

(8) In inclining the visual plane upward or toward the brows, if the optic convergence be strong the inclination of the horopteric line increases; but if the optic convergence be small it decreases, but does not reach zero or become perpendicular*.

(9) In looking downward 45°, for all distances the horopter is a *surface* passing through the point of sight and perpendicular to the median line of sight; but the form of the surface I have not attempted to determine. In looking straight forward at infinite distance, the horopter is also a *surface* passing through the point of sight; but the inclination of this surface I am unable to determine.

(10) It is possible that in some eyes which would be considered normal there is, in convergence, a rotation of the eyes *inward*, probably from greater power in the *superior* oblique. In such cases the position of the horopter would be different.

Columbia, S.C.,
November 16, 1868.

XXI. *On the Formation of Bubbles of Gas and of Vapour in Liquids.* By Charles Tomlinson, *F.R.S., F.C.S.*†

In the fifth Number of Poggendorff's *Annalen* for the present year, dated May 31, and published, I suppose, early in June, is a paper by Herr Schröder on the conditions under which bubbles of gas and of steam are formed in liquids‡. The paper is dated "Mannheim im December 1868," and a continuation is promised for a future Number. In paragraph 4, which is devoted to the history of the subject, the author does me the honour of referring to two papers of mine which appeared in the Philosophical Magazine just two years ago§, although he says he was not aware of the existence of my papers nor of those of M. Gernez‖, until he had completed the greater part of his researches on this subject. Still he does not think it superfluous to publish his paper, since he believes it will add new results to those obtained by M. Gernez and myself.

* See this statement modified in note on p. 202.

† Communicated by the Author.

‡ "Untersuchungen über die Bedingungen, von welchen die Entwicklung von Gasblasen und Dampfblasen abhängig ist, und über die bei ihrer Bildung wirksamen Kräfte," p. 76.

§ "On the so-called 'Inactive' Condition of Solids," Phil. Mag. for August and September 1867.

‖ *Comptes Rendus* for 1866 and 1867.

I am not aware whether Herr Schröder has seen my subsequent papers on the subject of which he treats*; but as he uses the same authorities, and no other, it is probable that he has. It cost me a considerable amount of research to find out the various memoirs of Œrsted, Schönbein, Liebig, and Gernez on the liberation of gases from solution under the influence of nuclei—of Watt and Southern, Achard, Gay-Lussac, Rudberg, Marcet, Bostock, Magnus, Donny, Grove, and Dufour on the phenomena of boiling liquids; and yet all these authorities, and no other, are made use of by Herr Schröder.

It is equally remarkable that Herr Schröder should use the terms "clean" and "unclean" in precisely the same sense that I do, in distinguishing between a body that is "inactive" in liberating gas or vapour from liquids and one that is "active" in doing so—and that he should describe an inactive body as being made active by drawing it through the "finger and thumb" (I say "the hand"), when it becomes contaminated with greasy or fatty matter which renders it active. It is also remarkable that Herr Schröder should have hit upon the same explanation of the action of flame, sulphuric acid, alkaline solutions, alcohol, &c. in rendering dirty bodies chemically clean, and therefore inactive as nuclei in gaseous and vaporous solutions.

I should have been quite content to leave all these matters unnoticed, seeing that priority of publication is in my favour, were it not that Herr Schröder claims for his distinguished countryman Schönbein the merit of first distinguishing in 1837 between an "inactive" and an "unclean" body in liberating gas.

Now in Schönbein's short paper† there is not the slightest evidence that the author had any idea whatever of the difference between clean and unclean bodies in liberating gas from solution. His theory was that solids acted by carrying down air, into which the gas in solution expanded and so got liberated. He expressly says that metals from whose surface the adhering film of air has been removed by dipping them into boiling water, do not disengage bubbles of steam from boiling liquids. Herr Schröder also makes Schönbein refer to the action of porous bodies as nuclei, whereas Schönbein does not even mention permanently porous bodies, such as charcoal, pumice, &c. He states, as Bostock had done twelve years before, that bits of wood are particularly

* "On some Effects of a Chemically Clean Surface," Phil. Mag. for October 1868.

"On the Action of Solid Nuclei in liberating Vapour from Boiling Liquids," Proceedings of the Royal Society for January 1869.

"Historical Notes on some Phenomena connected with the Boiling of Liquids," Phil. Mag. for March 1869.

"On Catharism, or the Influence of Chemically Clean Surfaces," Journal of the Chemical Society for April 1869.

† Pogg. *Ann.* vol. xl. p. 391.

active so long as their pores are full of air, but when, by long boiling or steeping, the air is expelled they become quite inert*.

Schönbein was by no means satisfied with the theory which attributed the action of solids in liberating gases or vapours from liquids to their carrying down air, a film of which was supposed to adhere to all bodies exposed to it; and he expressed his opinion that any one would perform an important service both to physics and to chemistry who could satisfactorily account for the varied phenomena connected with the subject of nuclei.

Although Herr Schröder had not seen the papers either of M. Gernez or myself, yet his own theory is a sort of compromise between the two. M. Gernez says that solids act as nuclei by carrying down air into which the gas in solution expands. I say that such solids act by a kind of differential force depending on the amount of adhesion between gas and an unclean body, and between water and an unclean body. The gas will adhere; the water, as a rule, will not; but when it does so, it is with diminished force. Herr Schröder says it is true that unclean bodies act because they are covered, more or less, with a film of fatty organic matter; but it is this film which enables the air to adhere to the solid, which adhering air, according to him and Gernez, is the efficient cause in liberating gas and vapour from liquids.

With respect to inactive or chemically clean solids made so by the action of flame, sulphuric acid, &c., I say that the supersaturated solution, whether of gas, of salt, or of steam or vapour, adheres to such solids as a whole (that is, there is the same force of adhesion between the gas, salt, or vapour and the solid, as between the liquid and the solid), and hence there is no separation. Herr Schröder says that the action of flame, sulphuric acid, &c. is to prevent the air from adhering to the solid, so that when inserted into the liquid there is no separation, because no air has been carried down into which the gas can expand.

It is not necessary for me to refer to any of the numerous experiments by which I justify my views on this interesting branch of inquiry. I do not ask Herr Schröder, or any one else, to adopt my theory or my experiments; they must go at their market price in the mart of science; but I do ask that when an observer takes up a subject which has been already handled, he should make himself acquainted with recent papers which, I suppose, are to be found in every public library; and when making use of scientific papers, whether old or new, that he acknowledge them and quote them fairly.

Highgate, N.,
August 11, 1869.

* Schönbein says, "Ganz besonders stark wirkt Holz so lange dessen Poren noch mit Luft angefüllt sind, aber gar nicht mehr, wenn diese ausgetrieben ist."

XXII. *On the Production of a Columnar Structure in Metallic Tin.* *By* Dr. T. FRITZSCHE *of St. Petersburg**.

THE occurrence of a curious structural change in block tin from Banca was observed by Dr. Fritzsche. The metal became crystalline, and fell into small pieces having a columnar form. This change was attributed to the intense cold prevailing in St. Petersburg at the commencement of the year 1868.

Dr. Fritzsche thus describes the experiments instituted to confirm his view:—"Although I was persuaded that this phenomenon was produced by the intense cold that we had at the beginning of 1868, I wished to prove it by experiments. These experiments I have lately completed. I exposed some fragments cut from a block of Banca tin in an alcohol-bath reduced to the temperature of $-32°$–$35°$ R. They underwent a change exactly similar to that in the blocks in question.

"It is necessary for a like cold to be sustained for some hours to induce the commencement of the crystallization, which showed itself by the appearance of button-like prominences of a steel-grey colour rising from the surface of the tin. Each prominence represents a centre from which the crystallization proceeds, if the cold be sustained. Gradually the meeting of the acicular crystals produces fissures at the points of contact, and the fragment, the volume of which is much augmented, falls in pieces, which are very friable and crumble between the fingers.

"A remarkable fact is that elevation of temperature causes the steel-grey colour to disappear. This may be shown by plunging the steel-grey tin (enclosed in a sealed glass tube) into hot water, when the natural white colour reappears but without the former metallic lustre. This change of colour is not attended by a loss of weight; neither is the transition of cast tin into the crystalline modification, in the presence of air or in alcohol, attended with any loss of weight. I have met with cavities in the altered blocks, one of which had a capacity of ·80 cub. centim.; I do not believe that such large cavities were formed during the cooling of the blocks. I attribute their formation to the act of crystallization; but on cutting these blocks I found that the change was only superficial, the centre being in the natural condition. I have there found similar cavities; and it is beyond doubt that they existed before the commencement of the change. As yet English tin has resisted the crystallization; but Banca tin also undergoes the change even after being melted.

"I shall continue my researches, as it is necessary to compare specific weights and to make analyses. I will communicate the ultimate results if they are of sufficient importance."

* From a letter to Mr. Graham, dated June 18, 1869. Communicated by Mr. Graham.

XXIII. *Fundamental Principles of Molecular Physics. Reply to* Professor Bayma. *By* Professor W. A. Norton.

[Continued from p. 41.]

After replying to the general remarks in the first part of my paper, Professor Bayma proceeds to the consideration of my answer to his criticisms of my original paper on 'Molecular Physics,' and ends by reaffirming his objections. I propose to examine briefly the more salient points in this portion of his elaborate reply.

Three Forms of Matter.—On this point we shall most readily get at the true state of the case by quoting the postulates in my original memoir bearing upon it. They are the following:—

"All bodies of matter consist of separate indivisible parts, called atoms, each of which is conceived to be spherical in form."

"Matter exists in three essentially different forms. These are (1) ordinary or gross matter, of which all bodies of matter directly detected by our senses either wholly or chiefly consist; (2) a subtile fluid or æther associated with ordinary matter, by the intervention of which all electrical phenomena originate or are produced. This *electric æther*, as it may be termed, is attracted by ordinary matter, while its individual atoms repel each other. (3) A still more subtile form of æther which pervades all space and the interstices between the atoms of bodies. This is the medium by which light is propagated, and is called the *luminiferous æther*, or the *universal æther*. The atoms or 'atomettes' of this æther mutually repel each other; and it is attracted by ordinary matter, and is consequently more dense in the interior of bodies than in free space."

In what sense the term form is here used would seem to be abundantly manifest. It is plain that the "three different forms" of matter are regarded as differing from each other in certain attributes which determine the precise office each fills in the scheme of Nature—and that the idea of a difference of geometrical form could not have been entertained, since it is distinctly asserted that all atoms are conceived to be spherical in form. In the next paragraph of my memoir I consider the question of the probable constitution of a single primitive molecule, and remark as follows:—"We are thus led to conceive of a molecule as consisting of an atom of ordinary matter surrounded with two atmospheres, æthereal and electric, the former being the more attenuated and pervading the other." The three "forms of matter," so called, are then the central atoms of molecules and the atoms of the two æthers. Each of these three general classes of atoms has certain characteristic attributes, in consequence of which their po-

sition and office in nature are different. Professor Bayma also recognized, in his 'Molecular Mechanics,' three distinct portions or general varieties of matter differing in certain attributes, viz. the attractive nucleus or "nuclei" of a primitive molecule, a repulsive "envelope" surrounding the nuclei, and the æther of space. In my reply to his criticisms, I stated that we agree in admitting the existence of *two kinds* of matter and *three forms* of matter. Thus, according to my view, ordinary or gross matter, *i. e.* ordinary material atoms or elements, constitutes one kind of matter, and æthereal matter another kind. The latter has the same fundamental properties, inertia, &c. as the former, but differs from it in some special property or attribute. Thus the atoms of ordinary matter were regarded as mutually attractive, and those of æthereal matter as mutually repulsive. It was also conceived that the active forces of the atoms of ordinary matter might be much less intense than those of æthereal matter—although the enormous difference between the elastic forces of the æther of space and of the electric æther and those in operation within bodies of ordinary matter might be wholly due to the fact that the latter forces are the reciprocal effective actions of molecules, which are differential, being the resultant of antagonistic actions. I will here take occasion to remark that the notion that the atoms of ordinary matter are mutually attractive, at first adopted, does not seem to be a necessary one; for if we regard them as mutually repulsive, it is conceivable that the attraction of gravitation might consist in a feeble excess in the attraction of the central atom of each molecule for the atmosphere of every other over and above the repulsion subsisting between the atmospheres of the two molecules, together with the corresponding excess in the attraction of the electric atmosphere of the first molecule for the central atom of every other over the repulsion subsisting between the central atoms of the two molecules. In fact the existence of the former excess is one of the theoretical deductions of my 'Molecular Physics.' Upon the view now taken, an atom of ordinary matter may differ from an æther-atom only in exerting a less energetic repulsion (in accordance with the theory propounded in my former answer to Professor Bayma), and in exerting a direct attractive action upon the atoms of the electric æther. The two æthers, which differ only in subtilety, and ordinary matter, as it has been defined, constitute the "three forms of matter."

With Professor Bayma the distinction between two kinds of matter lies wholly in the kind of activity manifested. The one kind is essentially attractive for all other elements, and the other essentially repulsive. He recognizes two varieties or forms of attractive matter—the molecular nuclei and the luminiferous

æther,—and one form of repulsive matter, viz. the molecular envelope.

If, after the explanation I have now given of my meaning in the phraseology used and of the conceptions actually formed, our author is still disposed to renew the question "on what evidence are we to grant that matter exists in three forms essentially different from each other," *i. e.* one attractive in the mutual action of its elements, and two repulsive in the same sense, or all repulsive in this sense, but exerting different intensities of repulsion, I will reply by asking *him* the same question, "on what evidence are we to grant that matter exists in three forms," viz. one repulsive and two attractive. If he should refer me to his 'Molecular Mechanics' for the evidence, I should respond by referring him to my 'Molecular Physics' for the evidence.

There is no occasion to add anything more on the question of the three forms of matter, except to remark that Professor Bayma's apparent success in exposing the "fallacy of my argument" about "gross matter" is attributable to the fact that he represents me as holding that gross matter is made up of molecules, whereas, as I have already shown in my conception and characterization of the three forms of matter, the gross or ordinary matter is simply the central atoms of the molecules. It may be as well to remark, also, that the term "gross matter" was adopted in conformity with common usage, in designation of what is universally called matter, without intending to imply that the atoms of necessity differed from the æthereal atoms, except in the intensity of their active forces as compared with the quantities of matter in the atoms.

Two Æthers.—It is asked, "Why two æthereal fluids when one might suffice." The "clear and positive answer" I have to give is this:—for the simple reason that, as I have endeavoured to show in my 'Molecular Physics,' from the conception of two æthers, the recognized molecular forces and the different classes of molecular phenomena in their diverse mutual relations and interdependence may be evolved, while all attempts to accomplish this result by means of the hypothesis of a single æther have signally failed. If our author or any other physicist will give us any substantial reason to believe that the notion of a single æther may really suffice to explain electric phenomena, we shall be ready to admit that his query throws a shadow of doubt on the hypothesis of two æthers. But we certainly cannot make the same admission in deference to his mere assertion that "one æther might suffice."

The proof, or, rather, strong evidence (which is all that the case admits of), that two æthers, both repulsive, exist in nature, is that optical and electric phenomena have given direct indications of

their existence, and the entire range of molecular phenomena can be shown to be deducible from their fundamental properties and relations to ordinary matter.

Electric Æther.—My critic still cherishes the illusion that a discrepancy or fallacy exists in my conception of the electric æther. It is true that in my original memoir I hinted that the *effective* mutual repulsion of the electric atoms might have its origin in a repulsion between æthereal atmospheres condensed around them by an attraction; but in my reply to Professor Bayma it was distinctly averred that I did not advocate this doctrine, and was only disposed to admit the possibility of its truth. If my pertinacious critic is still disposed to run a tilt against it under the hallucination that it is one side of my citadel, I can only look upon his adventure with the same sort of interest with which we contemplate the exploits of a knight-errant in a romance.

I will take occasion in this connexion to remark that the conviction entertained by our author and other eminent physicists, that the supposed electric fluid or æther is not to be regarded as a *vera causa* in nature, appears to have its origin in certain misconceptions or groundless assumptions.

(1) It is deemed more philosophical to seek for the true origin of electric and kindred phenomena in some mode of motion of the ultimate parts of bodies, notwithstanding that the existence of an æther (the luminiferous) having the same character of subtilety and enormous energy of elastic force as the supposed electric æther is distinctly recognized. This is as much as if, after Cavendish had discovered the properties of hydrogen gas, and the phenomena exhibited by oxygen had been carefully studied, it had been insisted that chemists must seek to explain these phenomena by some imagined modification of the mechanical condition of hydrogen, instead of attributing them to a new gas having certain specific differences of property from oxygen. Why should the hypothesis of a new æther similar to the luminiferous be regarded as inherently less probable than several hypothetical motions of the atoms or molecules of ordinary matter.

(2) It is imagined to be a simpler conception to refer electric phenomena to some mode or modes of motion of the atoms or elements of bodies than to a new æther. Atoms may be conceived to have any one of three different motions, viz. a vibratory motion, a motion of rotation, or a motion of revolution. Now let any one of these motions be hypothetically taken, and the attempt made to obtain some glimpse of the manner in which the phenomena might possibly evolve themselves. In the first place, there must be two different motions answering to the positive and negative electric states. In the next place, these motions must be capable of propagation from molecule to molecule

without changing their character, to represent a current of free electricity. Again, they must be capable of propagation from molecule to molecule with a continued reversion of their character, to explain the phenomena of induction. Still, again, these atomic or molecular motions must take place simultaneously with some other mode of motion, answering to heat, and another, representative of the magnetic or diamagnetic condition; and these different modes of motion must be convertible each into every other, &c. So far from being led into a region of attractive simplicity, the complexity of the scene that presents itself to the mind's eye would seem to be enough to appal the most determined explorer in the field of speculative science.

(3) It is conjectured by some physicists that the luminiferous æther may be equal to the duty assigned to the electric. But no approximation to a successful attempt has yet been made to realize this idea. It is a mere conjecture, and therefore unworthy of serious regard.

My own position on the question of the existence of an electric æther was not, as intimated by Professor Bayma, that it is an established truth, at least with the same degree of certainty that the existence of a luminiferous æther is, but an hypothesis (and the only definite hypothesis hitherto suggested by electric phenomena) which had been shown to be in accordance with the entire range of such phenomena, and thus come to be generally received. If it be true, as I maintain, that the molecular forces and molecular phenomena generally, in all their interdependence and mutual convertibility, can be derived from this hypothesis, when this shall come to be acknowledged it will then be admitted that full confirmation of the principle reached by induction has been furnished by the deductive test. The existence of an electric æther will then become an established truth in the most positive sense in which this can be affirmed of any principle in physics.

Origin of Heat.—In expressing the strong conviction that heat does not originate in the vibrations of gross molecules, reference was had to vibrations of the molecules as a whole, to the one side and the other of the positions of equilibrium. What is meant by "vibrativity" I do not fully comprehend. If we are to understand by it an alternate contraction and expansion of the repulsive envelope of a molecule, then Professor Bayma's theory of heat bears a certain analogy to my own, and may, for all that appears to the contrary, be free from the objections that may be urged against the doctrine that heat consists in a true vibration of atoms or molecules.

Luminiferous Æther.—There need be no hesitation as to the proper answer to be made to our author's argument to show that

the discovery made by Encke, that the comet which bears his name affords decisive evidence of the existence of a resisting medium in the fields of space, is really no discovery at all. In the first place, the attraction of unknown bodies would in all probability produce effects not recognized in the disturbed motions of Encke's comet—for example, would alter the position of the plane of the orbit. In the second place, Professor Bayma's mechanics is at fault; for though the direct tendency of the resistance of the supposed medium is to diminish the orbital velocity, a resulting effect is that the orbit is contracted, and the return of the comet to its perihelion expedited. This is Encke's view of the matter, and it has hitherto met with general acceptance. The words that issue from the filmy trumpet of this unwearied celestial traveller on each successive return have, then, quite a different meaning from those attributed to them by our author, and proclaim the insufficiency of the foundation on which his doctrine of an attractive æther has been erected.

As to Professor Bayma's comments on the objections urged against this doctrine, I think it must be admitted by the candid reader that the evidence in favour of my view of the constitution of a primitive molecule has been in no degree impaired by his criticisms. His idea that "no possible production of heat and electric currents affords a sufficient ground for assuming a reduction of resistance and retardation" is altogether fallacious; for if the impinging atoms of the æther of space take effect directly upon dense electric or æthereal atmospheres enveloping the atoms of gross matter, they may give rise to waves and currents in those atmospheres, propagated thence to other molecular atmospheres, and the energy conveyed by them eventually radiated in waves of heat through the interstitial æther and into free space from all sides of the atoms, and with no less intensity from the further sides than from those in advance. A similar principle to this is admitted in the theory of overshot water-wheels, when it is assumed that the mechanical effect due to the water received into the cell is lost—not communicated to the wheels—being expended primarily in imparting agitations on waves and currents to the water already in the cell, and eventually passing off in the form of heat. The state of the case then is this: the resistance of an æthereal medium in space will not of necessity retard the motions of the planets, if their atoms be surrounded by dense æthereal atmospheres, as I have been led to conceive them to be, on quite different grounds.

We come now to consider the answer given to my objection to Professor Bayma's doctrine of an attractive medium, viz. that it really involved the operation of an energetic resistance. I freely admit the sufficiency of his answer, if it follows from his

views that the repulsive envelope of each molecule must "beat back" the æther of space which it encounters before it comes within the range of the attraction of the central "nuclei." But does he not, in thus escaping one difficulty, encounter another equally great? This "beating back" of the æther implies that the molecules of the earth's mass in the advance are, by reason of the earth's motion, at such a diminished distance from the æthereal atoms immediately contiguous to them that a repulsive action of the molecular envelopes upon these atoms comes into play superior to that due to the condition of equilibrium that would obtain if the earth were at rest. If this be admitted, it must then at the same time be admitted that the molecules on the following side of the earth are at a corresponding increased distance from the æthereal atoms immediately behind them. If, then, the atoms of the æther are attractive, as our author maintains, since they are in closer proximity to the envelopes of the molecules of the earth on its preceding than on its following side, the attraction exerted by the æther upon the molecules must be more energetic on the former than on the latter side of the earth, and hence *the earth should be accelerated in its motion through space by the operation of the attractive æther supposed.* I must therefore conclude that the logical necessity still exists of "abolishing the æther of space altogether."

"*A Molecule.*"—The position called in question under this head had a phenomenal bearing only, as is sufficiently evident from the expression "in all outward relations," and the subsequent allusion to the production of phenomena. I was well aware that his "molecule" was, in the details of its constitution, quite different from my own—and in another connexion alluded to the multiplicity of assumptions made by the learned author of the 'Molecular Mechanics' in fashioning so complex and artificial a structure, and urged the objection that if we admit his conception of matter and of the several material activities, we still require the miraculous interposition of the Creator in the construction of every individual molecule in the universe. The ground taken was that in the evolution of phenomena, the nucleus or "nuclei" and envelope must each play, to all intents and purposes, the parts I had assigned to the central atom and electric atmosphere of my own molecule. If Professor Bayma is not disposed to admit this, I shall await with curiosity the further development of his theory, when I shall be in a position to decide with certainty how far I may have been in error in taking the ground just mentioned.

[To be continued.]

XXIV. *On a Remarkable Structural Appearance in Phosphorus.* *By* CHARLES TOMLINSON, *F.R.S., F.C.S.**

THE following remarkable appearance in phosphorus was described to me some months ago by Mr. James John Field, F.C.S., who requested me, if possible, to account for it.

About four years ago Mr. Field placed half a dozen sticks of phosphorus in a cylindrical jar containing water which rose about half an inch above the ends of the sticks, and the jar was closed with a bung. This jar was placed in a cellar, where it remained undisturbed for about three years. The cellar is flagged with stone, is surrounded by damp walls, and almost entirely protected from light and currents of air. The maximum temperature probably does not exceed 50° or 55° F.

After this long repose the jar was taken into the laboratory, when it was found that the level of the water had sunk to about one-third of its original height, and the liquid left in the jar had become as dense and thick as the strongest syrup; it consisted of a solution of PO^3 and PO^5.

The portions of phosphorus that rose some inches above the liquid, instead of being cylindrical as before, were conical from a sharp point to the full diameter, and each cone had a double spiral running down it from left to right, as if two flat tapering bands of the substance had been made to cohere at right angles lengthwise, and then twisted into a pointed rod—or just as if the sticks had been mounted in a screw-cutting lathe, geared to cut a coarse tapering double spiral. The sticks had also changed from the creamy opaque surface to a translucent barley-sugar appearance from the surface of the liquid up to the points.

In attempting to explain the appearances described, we must consider, *first*, the wasting away of the sticks and their conical form, and, *secondly*, the twisted structure.

First. The wasting away of the sticks and their conical form are clearly effects of slow combustion, diminishing in intensity downwards. The continued combustion and also the evaporation of the water must have been due to a badly fitting cork which, during a falling barometer, allowed a portion of the moist air to escape from the jar, and during a rising barometer allowed a portion of comparatively dry air to stream in. Had the jar been subject to considerable variations in atmospheric temperature, the effects would have been more rapid; but as the temperature of the cellar was pretty constant, there is nothing to detain us here. Going back, then, to variations in atmospheric pressure, the level of the water in the jar would be gradually lowered

* Communicated by the Author, having been read at the British Association at Exeter, August 19, 1869.

during the oscillations of the barometer, until at length the tops of the sticks of phosphorus became exposed. Slow combustion would then set in, the resulting acid would go into solution, and small quantities of fresh air would stream in to supply the partial vacuum, and so continue the action. During a falling barometer nitrogen and moisture would stream out of the jar, the level of the water would be again slightly lowered, and a fresh portion of phosphorus be exposed to the attacks of the next portion of oxygen drawn in. In this way by very slow degrees the liquid would be lowered and fresh portions of phosphorus exposed. Those already out of the water would be attacked by every ingress of air, and thus being acted on not only more energetically, but also for a longer time than the lower portions, they would necessarily have a conical shape. Moreover the air that streamed into the jar would gradually lose its oxygen in descending, so that the lower portions would be acted on less strongly than the upper. The phosphoric acids as generated would also pass into solution with a certain rise of temperature and a certain expansion of the nitrogen left in the jar. As this cooled down, a little more air would be drawn in, and combustion and solution would go on as before. But the most energetic action would take place when under a falling barometer a quantity of moist nitrogen streamed out of the jar, and during a rising barometer a fresh supply of atmospheric air streamed in, as already explained.

Secondly, as to the spiral markings. These cannot have been formed by any action that took place in the jar; but they show, I think, the new and interesting fact that the curves which the theory of hydraulics assigns to liquids flowing from an orifice, and producing the *vena contracta*, actually form part of the structure of a body suddenly arrested in its flow by being made solid.

It is well known that in the ordinary manufacture phosphorus is formed into sticks by being made to flow from a *head* or reservoir of the molten element along a short pipe or *ajoutage* into cold water; or, rather, as soon as the stick of phosphorus begins to emerge from the warm ajoutage and shows itself in the cold bath, it is seized by hand and cut off at intervals, or drawn out by machinery into a continuous length, so that from 15 to 20 lbs. and upwards of phosphorus can be moulded in less than a quarter of an hour.

Now, of course, in the flow of the molten phosphorus Torricelli's theorem applies, viz. that particles of fluid on escaping from an orifice possess the same velocity as if they had fallen freely *in vacuo* from a height equal to that of the fluid-surface above the centre of the orifice. If the head of phosphorus were

not too deep, there would be seen immediately over the orifice a hollow depression which increases until it becomes a cone or funnel the centre or lowest point of which is in the orifice, and the liquid flows in lines directed towards the centre. In this condition of the liquid a rotatory motion is necessarily imparted to it; and this rapidly increases, because all the particles are approaching the centre, and by virtue of their inertia they tend to maintain the same velocity which they had in a larger circle, so that their angular velocity (or the number of revolutions in a given time) is constantly being increased. As the particles approach the orifice they converge to a point beyond it, so that the liquid in escaping is narrower or more contracted at the point to which it converges than it is either before it arrives at that point, or after it has passed it. But as this point in the phosphorus to which the rotating lines converge, though fixed in or near the tube, is being constantly shifted in the phosphorus by being drawn out and moulded in the tube, the converging lines are also drawn out, and thus give the appearance of a double spiral. Of course some of the lines are obliterated by the moulding action of the tube, and are probably of a different texture as to hardness as compared with the drawn-out lines. These flattened or moulded portions first yield to the action of slow combustion, and leave the harder drawn-out lines in relief.

Highgate, N.,
July 31, 1869.

XXV. *On the Supposed Action of Light on Combustion.* *By* Charles Tomlinson, *F.R.S., F.C.S.**

THE popular idea that "light puts out the fire" is so fixed, that probably no conclusions drawn from actual experiment are likely to disturb it, especially if they be adverse to the notion. It is a matter of daily experience, people say, that if the fire is nearly out and you put a screen before it, or draw down the blind, or close the window-shutters, it will immediately begin to revive. It is generally forgotten that a fire which looks dull or "out" in a well-lighted room will appear to be in tolerable condition in the same room when darkened. It only requires to be "put together" to make it burn up, and it might have done so just as well in the light.

Experiments on this subject are not easy to make, on account of the many disturbing causes. In an old volume of the 'Annals of Philosophy' is an account of some experiments by Dr. M'Keever, who took two portions of green wax taper, each

* Communicated by the Author, having been read at the British Association at Exeter, August 20, 1869.

weighing ten grains, and ignited both at the same moment. One piece was placed in a dark room at 67° F., the other was exposed to broad sunshine at 78° F. In five minutes

The taper in sunshine lost $8\frac{1}{2}$ grains.
The taper in the darkened room lost $9\frac{1}{4}$ grains.

The taper, divided into inches, was also burnt in the coloured portions of the solar spectrum, when it was found that the time required to burn two inches of taper varied as follows:—

	′	″
In the red ray it took . . .	8	0
In the green ray it took . . .	8	20
In the violet ray it took . . .	8	39
At the verge of the violet it took	8	57

The conclusion is that the solar rays, in proportion to their intensity, have the power of retarding to a considerable extent the process of combustion; and it is supposed that the chemical rays act in some way on the portion of oxygen about to combine with the fuel so as to delay, if not prevent, combination.

Supposing in these experiments the taper was so uniform that one inch contained precisely the same quantity of matter as another inch, the time occupied in burning was too short to justify so important a conclusion as Dr. M'Keever arrived at, whether the results were taken by measure or by weight.

Every one engaged in photometrical observations must be aware of the difficulty of getting rid of disturbing causes and perplexing results. In comparing candles of the same make, the light is affected both in quantity and economy by a number of small circumstances, such as the warmth of the room, the existence of slight currents of air, the extent to which the wick curls over when burning, and so on. In testing the quality of gas, the standard candle defined by Act of Parliament is a sperm candle of six to the pound, burning at the rate of 120 grains per hour. From such a standard we get the terms "12-candle gas," "14-candle gas," &c. Mr. Sugg, in his 'Gas Manipulation,' has pointed out some of the difficulties in obtaining a uniform standard candle. The wick does not always contain the same number of strands; they are not all twisted to the same degree of hardness; the so-called sperm may vary in composition, one candle containing a little more wax than another, or variable quantities of stearine, or of paraffine; the candle may have been kept in store a long or a short time; the temperature of the store-room may have varied considerably, and the temperature of the room in which it was burnt may have been high or low. All these circumstances affect the rate of combustion and the illuminating-power of candles, irrespective of the action of light, if such action really exist.

I have lately had a good opportunity of testing this action at the works of Price's Patent-Candle Company at Battersea. Under the direction of Mr. Hatcher, the accomplished chemist of the Company, the greatest possible care is taken to ensure identity of composition and illuminating-power in candles of the same name. There has lately been an extensive series of experiments on the photometrical value of sperm candles, during which, at my request, Mr. Hatcher was good enough to note the rate of combustion of such candles in a darkened room, and also in broad daylight and even in sunshine.

In the first observation, three hard and three soft candles were burned each for four hours in a dark closet. A similar set of candles taken from one and the same filling were burned during the same time in open daylight, partly in sunlight. The average consumption per hour of each candle was as follows:—

Sperm in the dark	131 grains.
Sperm in the light	141 ,,
No. 2 Composites in the dark.	133 ,,
,, Composites in the light.	140 ,,

It must be noticed that the temperature in the light was 72°, and in the dark 71°. Moreover in the light there was a much greater motion of the air than in the dark closet. Both these circumstances would operate in producing a larger consumption of candle.

In a second trial with No. 2 composites the results were:—

In the dark . . .	140 grains each candle.
In the light . . .	134 ,, ,,

In a third, also with No. 2 composites, the results were:—

In the dark . . .	131 grains.
In the light . . .	129 ,,

In these two trials the flames were protected as far as possible from currents of air, and in the third trial the temperature both in the light and in the dark was nearly equal.

The fourth trial was made on a bright sunshiny day with hard sperm candles, which are less affected by variations of temperature than the composites. The results were—

In the dark (temp. 81°) . . 544 grains,

or 136 grains per hour.

In the light (temp. 84°) . . 567 grains,

or 142 grains per hour nearly.

It is evident that in this case the increase of temperature caused by the bright sunshine led to an increased consumption of material.

It will be seen that in the first and fourth trials there is a greater consumption of material in the light than in the dark, and in the second and third trials the consumption is greater in the dark than in the light; but in any case the difference is so small, amounting only to from 2 to 7 grains per hour, that it may fairly be referred to accidental circumstances, such as differences in temperature, in currents of air, and in the composition and make of the candles, the final conclusion to which I am led being that the direct light of the sun or the diffused light of day has no action on the rate of burning, or in retarding the combustion of an ordinary candle.

Highgate, N.,
July 1869.

XXVI. *On the Opinion that the Southern Hemisphere loses by Radiation more Heat than the Northern, and the supposed Influence that this has on Climate. By* JAMES CROLL, *of the Geological Survey of Scotland*.*

THE total amount of heat received from the sun between the two equinoxes is the same in both halves of the year, whatever the eccentricity of the earth's orbit may be. For example, whatever extra heat the southern hemisphere may at present receive from the sun during its summer months owing to greater proximity to the sun, is exactly compensated by a corresponding loss arising from the shortness of the season; and, on the other hand, whatever deficiency of heat we in the northern hemisphere may at present have during our summer half year in consequence of the earth's distance from the sun, is also exactly compensated by a corresponding length of season.

But the surface-temperature of our globe depends as much upon the amount of heat radiated into space as upon the amount derived from the sun, and it has been thought by some that this compensating principle holds only true in regard to the heat directly received from the sun. In the case of the heat lost by radiation the reverse is supposed to take place. The southern hemisphere, it is asserted, has not only a colder winter than the northern in consequence of the sun's greater distance, but it has also a longer winter; and this extra loss of heat from radiation is not compensated by its nearness to the sun during summer months, for it gains no additional heat from its proximity. And on the same principle our winter in the northern hemisphere, owing to the less distance of the sun, is not only warmer than that of the southern hemisphere, but is also at

* Communicated by the Author.

the same time shorter. Consequently it is concluded our hemisphere is not cooled to such an extent as the southern, and thus the mean temperature of the winter half year, as well as the intensity of the sun's heat, is affected by a change in the sun's distance.

This circumstance was, so far as I am aware, first noticed by Humboldt in his memoir "On Isothermal Lines and the Distribution of Heat over the Globe"*. Upon it M. Adhémar has founded a theory of change of climate, and attributes the great extension of the ice around the south pole to this extra amount of heat lost by radiation in consequence of the seven or eight days of excess in the length of the southern winter over the northern. "The south pole," says Adhémar, "loses in one year more heat than it receives, because the total duration of its nights surpasses that of the days by 168 hours; and the contrary takes place for the north pole. If, for example, we take for unity the mean quantity of heat which the sun sends off in one hour, the heat accumulated at the end of the year at the north pole will be expressed by 168, while the heat lost by the south pole will be equal to 168 times what the radiation lessens it by in one hour, so that at the end of the year the difference in the heat of the two hemispheres will be represented by 336 times what the earth receives from the sun or loses in an hour by radiation" †.

Adhémar supposes that about 10,000 years hence, when our northern winter will occur in aphelion and the southern in perihelion, the climatical conditions of the two hemispheres will be reversed; the ice will melt at the south pole, and the northern hemisphere will become enveloped in one continuous mass of ice, leagues in thickness, extending down to temperate regions.

Although I always regarded this cause of Humboldt's to be utterly inadequate to produce such effects as those attributed to it by Adhémar, still in former papers ‡ I stated it to be a *vera causa* which ought to produce some sensible effect on climate. On a more careful consideration of the whole subject, I now feel inclined to suspect that the circumstance in question can, according to theory, produce little or no effect on the climatic condition of our globe.

The rate at which the earth radiates into space the heat received from the sun depends upon the temperature of its surface; and the temperature of its surface (other things being equal) depends upon the rate at which the heat is received. The greater the rate at which the earth receives heat from the sun, the greate

* Edinb. Phil. Journ. vol. iv. p. 262 (1821).

† *Révolutions de la Mer*, p. 37 (second edition).

‡ Phil. Mag. S. 4. vol. xxviii. p. 131. Reader, December 2, 1865.

will therefore be the rate at which it will lose that heat by radiation. The total quantity of heat received during winter by the southern hemisphere is exactly equal to that received during winter by the northern. But as the southern winter is longer than the northern, the rate at which the heat is received during that season must be less on the southern hemisphere than on the northern. Now this less rate, were it not for a circumstance presently to be noticed, ought exactly to compensate for the longer winter. The southern hemisphere loses heat during a longer period than the northern; but then it does not lose it so rapidly. Therefore the total quantity of heat lost, were it not for the circumstance alluded to, would be the same on both hemispheres. The same mode of reasoning is equally applicable to the summers of the two hemispheres. The southern summer is shorter than the northern; but the heat is more intense, and the surface of the ground kept at a higher temperature; consequently the rate of radiation into space is greater.

When the rate at which a body receives heat is increased, the temperature of the body rises till the rate of radiation equals the rate of absorption, after which equilibrium is restored; and when the rate of absorption is diminished, the temperature falls till the rate of radiation is brought to equal that of absorption.

But notwithstanding all this, owing to the slow conductivity of the ground for heat, more heat will pass into it during the longer summer of aphelion than during the shorter one of perihelion; for the amount of heat which passes into the ground depends on the length of time during which the earth is receiving heat, as well as upon the amount received. Also in like manner during the longer winter in aphelion, more heat will pass out of the ground than during the shorter one in perihelion. Suppose the length of the days on the one hemisphere (say the northern) to be 23 hours, and the length of the nights, say, 1 hour; while on the other hemisphere the days are 1 hour and the nights 23 hours. Suppose also that the quantity of heat received from the sun by the southern hemisphere during the day of 1 hour to be equal to that received by the northern hemisphere during the day of 23 hours. It is evident that although the surface of the ground on the southern hemisphere would receive as much heat from the sun during the short day of 1 hour as the surface of the northern hemisphere during the long day of 23 hours, yet, owing to the slow conductivity of the surface for heat, the amount absorbed by the ground would not be nearly so much on the southern hemisphere as on the northern. The temperature of the surface during the day, it is true, would be far higher on the southern hemisphere than on the northern, and consequently the rate at which the heat would pass into the ground would be

greater on that hemisphere than on the northern; but notwithstanding the greater rate of absorption resulting from the high temperature of the surface it would not compensate for the shortness of the day. On the other hand, the surface of the ground on the southern hemisphere would be colder during the long night of 23 hours than it would be on the northern during the short night of only 1 hour; and the low temperature of the ground would tend to lessen the rate of radiation into space. But the decrease in the rate of radiation would not compensate fully for the great length of the night. The general and combined result of all those causes would be that a slight accumulation of heat would take place on the northern hemisphere and a slight loss on the southern. But this loss of heat on the one hemisphere and gain on the other would not go on accumulating at a uniform rate year by year, as Adhémar supposes.

Of course we are at present simply considering the earth as an absorber and radiator of heat, without taking into account the effects of distribution of sea and land and other modifying causes, and are assuming that everything is the same in both hemispheres, with the exception that the winter of the one hemisphere is longer than that of the other.

What, then, is the amount of heat stored up by the one hemisphere and lost by the other? Is it such an amount as to sensibly affect climate?

The experiments and observations which have been made on underground temperature afford us a means of making at least a rough estimate of the amount. And from these it will be seen that the influence of an excess of seven or eight days in the length of the southern winter over the northern could hardly produce an effect that would be sensible.

Observations were made at Edinburgh by Professor J. D. Forbes on three different substances, viz. Sandstone, Sand, and Trap-rock. By calculation, we find from the data afforded by those observations that the total quantity of heat accumulated in the ground during the summer above the mean temperature was as follows:—In the sandstone-rock the quantity accumulated was sufficient to raise the temperature of the rock 1° C. to a depth of 85 feet 6 inches. In the sand the quantity was sufficient to raise the temperature 1° C. to a depth of 72 feet 6 inches. And in the trap-rock the quantity stored up would only suffice to raise the temperature 1° C. to a depth of 61 feet 6 inches.

Taking the specific heat of the sandstone per unit volume as determined by Regnault, at ·4623, and that of sand at ·3006, and trap at ·5283, and reducing all the results to one standard, viz. that of water, we find that the quantity of heat stored up

in the sandstone would, if applied to water, raise the temperature of the water 1° C. to a depth of 39 feet 6 inches; that stored up in the sand would raise the temperature of the water 1° C. to a depth of 21 feet 8 inches, and that stored up in the trap would raise the water 1° C. to the depth of 32 feet 6 inches. We may take the mean of these three results as representing pretty accurately the quantity stored up in the general surface of the country. This would be equal to 31 feet 3 inches depth of water raised 1° C. The quantity of heat lost by radiation during winter below the mean was found to be about equal to that stored up during summer.

The total quantity of heat per square foot of surface received by the equator from sunrise till sunset at the time of the equinoxes, allowing 22 per cent. for the amount cut off in passing through the atmosphere, is 1,780,474 foot-pounds. In the latitude of Edinburgh about 938,160 foot-pounds per square foot of surface is received, assuming that not more than 22 per cent. is cut off by the atmosphere. At this rate a quantity of heat would be received from the sun in two days ten hours (say, three days) sufficient to raise the temperature of the water 1° C. to the required depth of 31 feet 3 inches. Consequently the total quantity of heat stored up during summer in the latitude of Edinburgh is only equal to what we receive from the sun during three days at the time of the equinoxes. Three days' sunshine during the middle of March or September, if applied to raise the temperature of the ground, would restore all the heat lost during the entire winter; and another three days' sunshine would confer on the ground as much heat as is stored up during the entire summer. But it must be observed that the total duration of sunshine in winter to that of summer in the latitude of Edinburgh is only about as 4 to 7. Here is a difference of two months. But this is not all; the quantity of heat received during winter is scarcely one-third of that received during summer; yet notwithstanding this enormous difference between summer and winter, the ground during winter loses only about six days' sun-heat below the maximum amount possessed by it in summer.

But if what has already been stated is correct, this loss of heat sustained by the earth during winter is not chiefly owing to the fact of the longer absence of the sun during winter, but to the decrease in the quantity of heat received in consequence of his longer absence combined with the obliquity of his rays during that season. But in the case of the two hemispheres, although the southern winter is longer than the northern, the quantity of heat received by each is the same. But supposing it held true, which it does not, that the loss of heat sus-

tained by the earth in winter is as much owing to the excess in the length of the winter nights over those of the summer as to the deficiency of heat received in winter from that received in summer, three days' heat would then in this case be the amount lost by radiation in consequence of this excess in the length of the winter nights. The total length of the winter nights to those of the summer is, as we have seen, about as 7 to 4. This is a difference of nearly 1200 hours. But the excess of the south polar winter over the north amounts to only about 184 hours. Now if 1200 hours give a loss of three days' sun-heat, 184 hours will give a loss of scarcely $5\frac{1}{2}$ hours.

It is no doubt true that the two cases are not exactly analogous; but it is obvious that any error which can possibly arise from regarding them as such cannot materially alter the conclusion to which we have arrived. Supposing the effect were double, or even quadruple, what we have concluded it to be, still it would not amount to a loss of two days' heat, which could certainly have little or no influence on climate.

But even assuming all the preceding reasoning to be incorrect, and that the southern hemisphere, in consequence of its longer winter, loses heat to the extravagant extent of 168 hours, supposed by Adhémar, still this could not materially affect climate. The climate is influenced by the mere *temperature* of the *surface* of the ground, and not by the quantity of heat or cold that may be stored up under the surface. The climate is determined, so far as the ground is concerned, by the temperature of the surface, and is wholly independent of the temperature which may exist under the surface. Underground temperature can only affect climate through the surface. If the surface could, for example, be kept covered with perpetual snow, we should have a cold and sterile climate, although the temperature of the ground under the snow was actually at the boiling-point. Let the ground to a depth of, say, 40 or 50 feet be deprived of an amount of heat equal to that received from the sun in 168 hours. This could produce little or no sensible effect on climate; for, owing to the slow conductivity of the ground for heat, this loss would not sensibly affect the temperature of the surface, as it would take several months for the sun's heat to penetrate to that depth and restore the lost heat. The cold, if I may be allowed to use the expression, would come so slowly out to the surface that its effect in lowering the temperature of the surface would scarcely be sensible. And, again, if we suppose the 168 hours' heat to be lost by the mere surface of the ground, the effect would certainly be sensible, but it would only be so for a few days. We might in this case have a week's frozen soil, but this would be all. Before the air had time to

become very sensibly affected by the low temperature of the surface the frozen soil would be thawed.

The storing up of heat or cold in the ground has in reality very little to do with climate. Some physicists explain, for example, why the month of July is warmer than June by referring it to the fact that by the month of July the ground has become possessed of a larger accumulation of heat than it possessed in June. This explanation is evidently erroneous. The ground in July certainly possesses a greater store of heat than it did in June; but this is not the reason why the former month is hotter than the latter. July is hotter than June because the air (not the ground) has become possessed of a larger store of heat than it had in June. And why the air is warmer in July than in June is this: it is with extreme difficulty that the air can become heated by the direct rays of the sun; it is by means of contact with the hot surface of the ground and by radiation from the earth that the air becomes slowly heated. Consequently, although the sun's heat is greater in June than it is in July, it is near the middle of July before the air becomes possessed of its maximum store of heat. We therefore say that July is hotter than June because the air is hotter in the former month than in the latter, and consequently the temperature in the shade is greater in the former month than in the latter.

If the distribution of sea and land were the same in both hemispheres, it follows, according to theory, that, owing to the excess of 184 hours in the length of the southern polar winter over the northern, there would be a very slight loss of heat on the southern hemisphere and a very slight gain of heat on the northern. But owing to the present distribution of sea and land, the very reverse in reality takes place. At present the northern hemisphere loses by radiation far more heat than the southern. The reason of this is obvious. The greater part of the southern hemisphere is occupied by sea. Water is a much worse radiator than land. There are a great many reasons for this, a few of which may be enumerated :—(1) The temperature of the surface of the water does not rise so high under the direct rays of the sun as that of the surface of the ground. (2) The heat-rays from the sun penetrate the water to a considerable depth, and in this case it is only a part of the heat that is received by the surface of the water, whereas in regard to land all the heat is received by the surface. The temperature of the surface of the land is thus raised enormously, and the heat rapidly thrown back into stellar space; this effect is also increased by the fact that the specific heat of the land is not one-half that of water. (3) The ground can only store up heat by the very slow process of conduction, whereas water, by the mobility of its particles and

transparency for heat-rays, especially those from the sun, becomes heated to a considerable depth rapidly. The quantity of heat stored up in the ground is comparatively small; the quantity stored up in the ocean is great. (4) The aqueous vapour of the air acts as a screen to prevent the loss by radiation from water, while it allows radiation from the ground to pass more readily into space. (5) The air is heated more rapidly by contact with the hot surface of the ground than it is by contact with the surface of the ocean. Consequently the heat which is carried up into the higher regions of the atmosphere and thrown off into stellar space chiefly comes from the land.

But it may be asked, If the southern hemisphere absorbs far more heat than the northern, why, then, is its mean temperature so much below that of the northern? The lower temperature of the southern hemisphere is evidently due, not to the loss of heat by radiation as supposed by Adhémar and others, but to a cause which has been completely overlooked, viz. to the normous amount of heat transferred from that hemisphere to the northern by means of ocean-currents.

The great ocean-currents of the globe take their rise in three immense streams from the Southern Ocean, which, on reaching the tropical regions, become deflected in a westerly direction and flow along the southern side of the equator for thousands of miles. A considerable portion of these currents returns into the Southern Ocean without ever crossing the equator, but the greater portion of them crosses over to the northern hemisphere. Since there is then a constant flow of water from the southern hemisphere to the northern in the form of surface-currents, it must be compensated by *undercurrents* of equal magnitude from the northern hemisphere to the southern. The currents, however, which cross the equator are far higher in temperature than their compensating undercurrents; consequently there is a constant transference of heat from the southern hemisphere to the northern. Any currents taking their rise in the northern hemisphere and flowing across into the southern are comparatively trifling, and the amount of heat transferred by them is also trifling. There are one or two currents of considerable size, such as the Brazilian branch of the great equatorial current of the Atlantic, and a part of the South Equatorial Drift-current of the Pacific, which cross the equator from north to south: but these cannot be regarded as northern currents; they are simply southern currents deflected back after crossing over to the northern hemisphere. The heat which these currents possess is chiefly obtained on the southern hemisphere before crossing over to the northern; and although the northern hemisphere may not gain any temperature by

means of them, it, on the other hand, does not lose much; for the heat which they give out in their progress along the southern hemisphere does not belong to the northern hemisphere.

But after making the fullest allowance for the amount of heat carried across the equator from the northern hemisphere to the southern, we shall find, if we compare the mean temperature of the currents from the southern hemisphere to the northern with the mean temperature of the great compensating undercurrent and the one or two small surface-currents, that the mean temperature of the water crossing from the southern hemisphere to the northern is very much higher than the mean temperature of the water crossing from the northern to the southern. The mean temperature of the water crossing the equator from south to north is probably not under 65° F., while the mean temperature of the undercurrent is probably not over 39° F. But we must add to them the surface-currents from north to south. And let us assume that this will raise the mean temperature of the entire mass of water flowing from north to south to, say, 45° F. Here we have a difference of 20° F. Each cubic foot of water which crosses the equator will in this case transfer about 1250 units of heat from the southern hemisphere to the northern. If we had any means of ascertaining the volume of those great currents crossing the equator, we should then be able to make a rough estimate of the total amount of heat transferred from the southern hemisphere to the northern; but as yet no accurate estimate has been made on this point. Let us assume, what is probably much below the truth, that the total amount of water crossing the equator is at least double that of the Gulf-stream as it passes through the Strait of Florida, which amount we have already found to be equal to 133,816,320,000,000 cubic feet daily*. Taking the quantity of heat conveyed by each cubic foot of water of the Gulf-stream at 1500 thermal units, it is found that an amount of heat is conveyed by the current equal to all the heat that falls within 63 miles on each side of the equator†. Then, if each cubic foot of water crossing the equator transfers 1250 thermal units, and the quantity of water is double that of the Gulf-stream, it follows that the amount of heat transferred from the southern hemisphere to the northern is equal to all the heat falling within 105 miles on each side of the equator, or equal to all the heat falling on the southern hemisphere within 210 miles of the equator. This quantity taken from the southern hemisphere and added to the northern will therefore make a difference in the amount of heat possessed by the two hemispheres equal to all the heat which falls on the southern hemisphere

* Phil. Mag. for June 1867, p. 433. Geol. Mag. for April 1869.
† Ibid. p. 434.

within somewhat more than 420 miles of the equator, supposing the sun to be vertical over the whole area.

This enormous difference is quite sufficient to account for the lower mean temperature of the southern hemisphere.

But it may be noticed that although the return currents at the equator are colder than the direct currents, yet they are not so in the polar regions. The water which leaves the polar seas is much colder than the water which replaces it from the tropical regions.

The general tendency of the great system of ocean-currents is to cool the equatorial region of the globe and to warm the temperate and polar regions. Also, owing to the present distribution of sea and land, and partly to the effects on the trade-winds resulting from the eccentricity of the earth's orbit*, small as that eccentricity is at present, there is a constant transference of heat by means of currents from the southern hemisphere to the northern. Ocean-currents tend to reduce the enormous difference of temperature which, according to theory, ought otherwise to exist between the equator and the poles†.

On a former occasion it was shown that aërial currents at the equator only tend to cool the equator; they do not carry heat to higher latitudes. But aërial currents in temperate and polar regions diffuse over the land the heat carried by ocean-currents. It is the ocean and not the air that conveys the heat from the tropics to the temperate and polar regions‡.

XXVII. *Description of some Lecture-experiments in Electricity.* *By* Professor G. C. Foster, *F.R.S.*§

THE object of this communication is simply to point out methods, differing somewhat from those commonly described in the books, of demonstrating two or three familiar truths of electricity. The experiments I am about to describe may probably be well known under one form or another, especially to practical electricians, who often have opportunities of using apparatus and witnessing phenomena which do not fall to the lot of mere scientific students. I do not claim for them any novelty, unless it be as lecture-room illustrations.

1. *Experiments with the Electrophorus.*—So far as I am aware, the experiments by which the accepted explanation of the action of the electrophorus is supported refer exclusively to the statical conditions of the instrument, or, in other words, to the states of

* Phil. Mag. S. 4. vol. xxviii. p. 135; vol. xxxiii. p. 122.

† Ibid. vol. xxxiii. p. 435; vol. xxxiv. p. 128.

‡ Ibid. vol. xxxiii. pp. 127–130. Geological Magazine for April 1869.

§ Communicated by the Author.

electrical equilibrium which it exhibits. The dynamical processes by which these statical conditions are brought about are no doubt, in their main features, very easily traced, and are perfectly well known; but, until quite recently, it has been a rare exception for electricians to be in possession of the instrumental means requisite for making them the subject of direct investigation. Now, however, the form of reflecting galvanometer devised by Professor Sir William Thomson is in the hands of a great many experimenters; and it accordingly seemed to me that, with the view of calling attention to the ease with which the transient electric currents accompanying the production and disappearance of electrostatical charge in various familiar cases can be observed, and even measured, by means of this instrument, it might be worth while to describe the following experiments.

An insulating table was made by laying a thin board across two insulated cylindrical conductors, such as are to be found in every collection of electrical apparatus. On this was placed a piece of sheet zinc, to serve as the lower plate of an electrophorus, the "cake" of which consisted of a circular piece of vulcanized india-rubber, about 15 inches in diameter and $\frac{1}{8}$ inch thick, and the "cover" of a circular brass plate 12 inches in diameter, with a glass handle. The lower metal plate was connected, by means of an insulated wire, with one terminal of a Thomson's astatic galvanometer having copper-wire coils of upwards of 6900 B.A. units resistance, the other terminal of which was connected with a gas-pipe in the laboratory, so as to make a good earth-contact. On rubbing the india-rubber with the hand, the cover having been removed, the galvanometer showed a deflection which, as soon as it had become steady enough to be read, amounted to 35 divisions of the scale on the side indicating the passage of a positive current from the earth into the electrophorus-plate. This deflection gradually diminished while the rubbing was continued, the spot of light finally returning to zero. The earth-wire was now removed from the galvanometer and replaced by a wire connected with the cover: on laying the cover upon the india-rubber, the galvanometer gave a deflection of 250 divisions on the opposite side to that observed during the rubbing. On lifting the cover again, there was a deflection of 230 divisions in the original direction, followed by a deflection of 200 to the other side on replacing it. On repeatedly lifting and replacing the cover, deflections were obtained every time, though gradually diminishing in amplitude in consequence of the imperfect insulation of the india-rubber.

In a second similar experiment, the maximum deflection during the rubbing was 40 divisions; the deflection on putting on the cover, 260 divisions; on removing it, 240.

These results show very plainly the nature and importance of the electrical changes which take place in the lower plate of the electrophorus while the apparatus is being used. Their meaning is too obvious to require further comment.

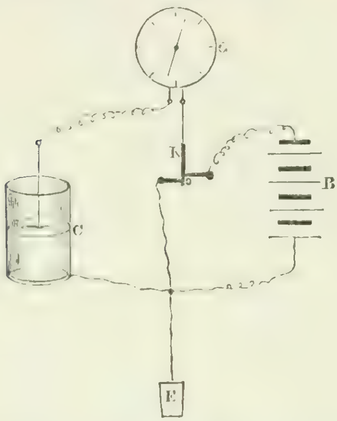
Equally decisive results are obtained if the lower plate is left constantly in connexion with the earth through the galvanometer, and the cover is repeatedly put on, touched, raised, discharged, and replaced, as in the common way of taking a series of charges from the electrophorus. On putting on the insulated cover, the galvanometer is not affected; but on afterwards touching the cover, a strong deflection is obtained in the direction indicating a downward positive current (that is, a current through the galvanometer into the ground). When the cover is raised, there is a deflection to the opposite side, indicating an upward positive current, which is again inverted if the cover be replaced without having been discharged; but, if it be touched before being replaced, no deflection is caused on putting it on again.

The importance of free electrical communication between the lower plate of the electrophorus and the earth is still further illustrated by the following experiments. First, the lower plate was insulated, both during the rubbing and afterwards, and the cover was connected through the galvanometer with the earth-wire: on now putting the cover on or taking it off by means of the glass handle, a deflection of from 5 to 10 divisions was obtained alternately on the two sides of zero. Next, the experiment was repeated, the india-rubber being rubbed the same number of times, in the same manner as before, but during the rubbing the lower plate was uninsulated; this time the deflection caused by putting on the cover amounted to 130 divisions, and on taking it off to 127.

A Thomson's galvanometer also serves very conveniently for proving the movement of electricity which takes place when a conductor is charged by statical induction. For example, one terminal of the galvanometer being connected to earth and the other with an insulated brass cylinder 2 inches in diameter and 17·5 inches long, a deflection of 10 or 12 divisions was obtained on bringing the slightly charged cover of the electrophorus near to the cylinder, and an equal deflection on the opposite side on removing it. These deflections, which might easily have been increased by using a body more strongly electrified, could be reproduced an indefinite number of times by simply moving the electrophorus-cover towards or away from the brass cylinder.

2. *Comparative Measurement of the Electrical Capacity of Conductors.*—The quantity Q of electricity which passes into or out of any insulated conductor, when put into electrical communication with any source of constant electrical potential, is pro-

portional to the difference of potential E between the insulated conductor and the source, and to a coefficient S called the electric capacity of the conductor and depending on the extent and disposition of its surface, and its position relatively to other conductors. This relation is very easily proved by means of a Thomson's galvanometer connected with a Leyden battery and a galvanic battery in the way shown in the figure.



G = galvanometer.
B = galvanic battery.
C = Leyden battery.
K = key.
E = earth-contact.

For example, a Leyden battery of six jars, each jar having a diameter of 18 centims. and being coated to a height of 24 centims. from the bottom, was charged and discharged through the galvanometer by four Grove's cells arranged in series. The sum of the deflections on both sides of zero, due to the charge and discharge, was (as the mean of several experiments) 88·8, the highest reading being 90, the lowest 88. When three of the jars were removed, so as to leave a battery of only half the previous capacity, the mean reading of several experiments was 45·1, the maximum being 45·5 and the minimum 44·5.

3. *Comparative Measurement of Electromotive Force.*—Precisely the same arrangement of apparatus and mode of experimenting that serves for comparing the capacities of conductors, also serves for comparing the electromotive forces of batteries; but, in order to make the comparison more accurate, it is advisable to substitute a conductor of greater capacity for the Leyden battery mentioned in the last paragraph, unless the electromotive forces to be compared are rather considerable. In the following experiments the condenser of a medium-sized Ladd's induction-coil was used.

When the condenser was charged and discharged through the galvanometer by one Grove's cell, the sum of the readings on

the two sides of zero was

252 divisions;

with two Grove's cells, the sum of the readings was

507 divisions.

	Divisions.
With one Daniell's cell, the sum of the readings was	152
With another Daniell's cell, it was	155
Sum	307

With the two Daniell's cells connected in series, the sum of the opposite deflections was 307 divisions.

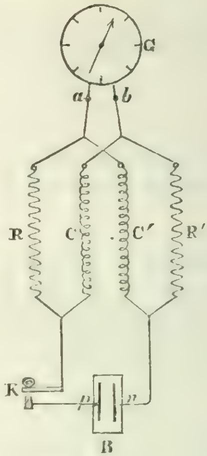
These numbers give, as the mean ratio of the electromotive force of one Grove's cell to that of one Daniell's cell,

$$507 : 307 = 1{\cdot}65 : 1.$$

According to Poggendorff, the ratio, as determined by his method of compensation, is 1·68 : 1.

The mode of comparison by means of the galvanometer and condenser may be rendered more accurate by increasing the capacity of the latter, so as to get larger readings and so diminish the relative importance of the errors of observation. The above numbers, however, which are of course given merely for the sake of illustration, do not represent the limit of accuracy attainable with the apparatus I employed: by simply altering the position of the adjusting magnet of the galvanometer, so as to render the suspended magnets more perfectly astatic, a deflection of 355 was obtained instead of 307. For proving to a class the way in which the electromotive force of a galvanic battery depends upon the mode in which the cells composing it are connected together, and other fundamental facts of a like nature, this method can easily be made abundantly accurate, and is probably as convenient and rapid as any of the methods in common use.

4. *Method of demonstrating the existence of the Inverse and Direct Extra-currents.*—The only method of rendering distinctly evident the retardation in the establishment of electric currents in coiled conductors, or Faraday's extra-current on making battery-contact, which I have found described in any of the ordinary text-books of physics, is one due to Edlund, and requires the use of a differential galvanometer. By an arrangement of apparatus, which may be regarded as a modification of that employed by Edlund, it is easy to show the extra-current both on making and breaking the circuit upon an ordinary galvanometer. This arrangement will be understood by reference to the figure, where



B represents a galvanic battery of one or two cells, K a key for making and breaking the battery circuit, G the galvanometer, C and C′ two coiled conductors, with or without iron cores, and R and R′ two zigzag or uncoiled conductors, of which the resistances are so adjusted relatively to the resistances of C and C′ that, when the battery-contact is permanently maintained, no current passes through the galvanometer. Then, on completing the circuit, there is a temporary deflection of the galvanometer due to the inverse extra-current, and on breaking it there is an opposite deflection due to the direct extra-current. The reason of this is easily seen. Supposing p to be the positive and n the negative pole of the battery, when the key K is pressed down the current is immediately established in the circuit B R a b R′ B, causing a corresponding deflection of the galvanometer; after a very short interval, however, the current is also established in the circuit B C b a C′ B, and brings the galvanometer-needle to rest. On raising the key the current ceases instantaneously in the uncoiled conductors R and $R_{\prime}$, but continues for a short time in the coiled conductors C and C′, traversing the galvanometer from b to a and causing a momentary deflection in the opposite direction to that produced on making the battery-circuit. Using for the conductors C and C′ the primary wire of a medium-sized Ladd's induction-coil and the wire of a straight electromagnet, and uncoiled German-silver wires for the conductors R and R′, I obtained with one cell of Grove's battery a swing of from 50° to 60° on a large astatic galvanometer with heavy needles 8 inches long on completing the battery-circuit, and an equal swing in the opposite direction on breaking contact after the needles had come to rest. The directions of the swings were such as to indicate that the current both commenced and ceased more suddenly in the uncoiled than in the coiled conductors.

The only special precaution that need be pointed out in order to ensure the success of this experiment, is that the resistances of the several conductors shall be so small, and their mass so great, that they may not become sensibly heated and so have their relative resistances changed during the passage of the current.

It will be seen that the combination of conductors that has been described is essentially the same as that constituting Wheatstone's "electrical balance;" in fact the whole experiment consists in purposely exaggerating an effect which, in comparing electrical resistances by means of that arrangement, it is necessary to get rid of by a well-known artifice in the mode of making contact.

XXVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 164.]

December 23rd, 1868.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the so-called 'Eozoonal' Rock." By Prof. W. King and Dr. T. H. Rowney. Communicated by Sir R. I. Murchison, Bart., K.C.B., F.R.S., V.P.G.S.

The authors noticed that, since the reading of their former communication in 1866, further descriptions of *Eozoon* have been published by Hochstetter, Gümbel, Carpenter, Dawson, and Logan; and after a few words on those by the first two, they proceeded to criticise the others more fully, intimating that the English and Canadian observers have by no means mastered all the difficulties of the subject, nor answered the objections brought forward by them. In the course of these remarks, Messrs. King and Rowney, objecting to the specimen from Tudor, of which they have seen the photograph, and which was described and figured in 1867 (Q. J. G. S. No. 91), suggested that it is nothing more than the result of infiltration of carbonate of lime, with entangled impurities, between two layers of the sandy limestone. They also stated their belief that the term "Eozoonal" is applicable to any of the ophites they describe, inasmuch as, it was contended, the structure of the latter is similar to that of the Canadian rock containing the so-called *Eozoon*.

The authors then proceeded to treat of the supposed *foraminiferal* characters of "Eozoon." First, as to the "cell-wall" or "nummuline layer," they advanced repeated evidence of the value of their former proofs that the typical form is due to aciculate serpentine (or modified chrysotile) of inorganic origin, having examined, besides others, a Canadian specimen presented by Dr. Carpenter. Secondly, nothing new was adduced with regard to the mineral structure of the so-called "intermediate skeleton." Thirdly, in proof that the "chamber-casts" are not of organic origin, the authors referred to their former work, and stated that chondrodite and pyrallolite may be added to the list of minerals that occur, as such, disseminated in limestones. They thought it strange that a carbonate, as well as a silicate, should not have been found filling the so-called

chambers; and they decidedly refused to accept the Tudor specimen having some tubuli filled with calcite, to which they suppose Dawson refers when speaking of chambers filled with calcite, as a case in point; they were unacquainted with any published instances of this mineral being an infilling. Fourthly, reiterating their observations on the so-called "canal-system," they suggested that the globoso-vermicular bodies noticed by Dawson and Gümbel may be metaxite; and they insisted on the difficulty of explaining the presence of isolated unbroken tube-casts in patches of pure limestone. The Madoc specimen, described by Dawson as having its "canals" and "chambers" filled with calcite, was next referred to: and it was argued that the so-called calcite, both in this and in another specimen, described by Carpenter, is doubtful and not proved; for they had not been able to confirm the accuracy of the observations in these cases, having examined a Canadian specimen, presented by Dr. Carpenter as an example of the kind, which had in it "homogeneous and structureless forms of the canal-system" that were not dissolved in the decalcification. Fifthly, the organic nature of the so-called "stolons" was regarded as quite disproved. *Mineralogical* considerations of Eozoonal rocks were next entered upon; and from the study of Canadian specimens, and of others from Connemara and Neybiggen (?), described in full, the authors concluded that they fully prove the "canal-system," "chamber-casts," and "nummuline layer" to be structural and inorganic modifications of serpentine—that the whole have originated from the change or waste of granules, plates, &c. of serpentine; and they incline to the belief that the calcite of the "intermediate skeleton" is pseudomorphic after one or other form of serpentine by infiltration and replacement. The rounded form of the granular masses of chondrodite, coccolite, &c. in some limestones was also referred by the authors to the gradual removal of their surfaces by deep-seated hydrothermal agency.

It was then argued that the organic nature of *Eozoon* cannot be supported by the cumulative evidence afforded by the combination of foraminiferal features; for these features, *combined* and due to purely mineral paragenesis, had occurred to the authors in certain ophites, though some are wanting in other ophites, just as they are not always present in the Eozoonal rock of Canada.

Serpentine has been described as having been deposited in the cavities of Eozoon, and having taken the place of its sarcode; but the authors criticised all the quoted analogies of such a precipitation of any siliceo-magnesian substance, disbelieved them, and put aside glauconitic infiltration as beside the question.

Considered *geologically*, with reference to its occurrence in a metamorphic rock, the authors regarded the *Eozoon* as an organic impossibility; and they asked why it should never be found in anything but crystalline or semicrystalline rocks—in ophites or ophicalcites of widely different ages. Particularly they found eozoonal structure in the Liassic ophite of Skye; and this they described in full. They criticised Sterry Hunt's change of opinion, who used to think

that the serpentinous rocks of Canada were once earthy amorphous silicates, and afterwards metamorphosed, but who now supposes they were deposited in a crystalline state; and they asked why, if so, may not all the Laurentian rocks have been so deposited? In conclusion, they totally denied that Eozoonal structure has anything to do with any organism; and repeated that, like all analogous conditions of serpentine, chondrodite, &c., it is of purely mineral origin.

Dr. Carpenter need not repeat the grounds on which he regarded this as an organic structure. He objected to criticisms unless founded on examination of actual specimens. Sir Wm. Logan had been first led to regard the *Eozoon* as organic by finding alternations of calcareous and siliceous layers in various minerals. A specimen which Sir William had brought from Canada contained much iron, and had the canal system wonderfully preserved; and it presented this character—that the larger branches were infiltrated with serpentine, and the middle branches with sulphide of iron, while the smallest branches were filled with carbonate of lime, of the same nature as the matrix. It was only under a favourable light that these smaller tubes were visible, as the calcite in them was of the same crystalline character as the surrounding network. This was conclusive evidence of the structure not arising from the mere infiltration of one chemical substance into another. Moreover this foreign matter could not penetrate the cleavage-planes.

When cut, some specimens had given out a strong odour of musk, which they to some extent still retained. This, again, seemed to be evidence of organic origin. He regretted that Prof. King had not examined the large collection of specimens in his (Dr. Carpenter's) collection. Recent Foraminifera, when decalcified, exhibited precisely the same asbestiform layer round the chamber-cast as the fossil *Eozoon*. Different genera of Foraminifera in recent seas were infiltrated by different minerals, which presented some analogy with the condition of the fossil under consideration. In the great seas of the present day, at various depths and temperatures, was a large extension of sarcodic substance, and in this there were Rhizopods with and without shells, but of similar low structure; and such forms might have continued in existence through any length of time, so that the occurrence of *Eozoon* so far down as Jurassic times could afford no matter for surprise. He would not be astonished even if such a structure as *Eozoon* were found in deep-sea dredgings of the present day.

The President mentioned the *Bathybius*, which he has found with coccoliths and other forms in deep-sea soundings. In some newer specimens of Atlantic mud given him by Dr. Carpenter he had found *Bathybius* forming a sort of network, somewhat similar to the plasmodia of botanists. He could not call it either plant or animal. It was, however, a living substance, susceptible of apparently indefinite growth. This removed one of the difficulties in believing in the wide extension of the *Eozoon*. The Hydrographer had since sent him the soundings taken by Captain Shortland in 'The Hydra.' In soundings from 2800 fathoms in the Arabian

Gulf *Bathybius* was plentiful; and over an area 7000 miles long the same organism occurred in abundance. He agreed in thinking it possible that such organisms might have gone on living from the earliest geological times.

In answer to Prof. Ramsay, the President stated that the soundings in which the *Bathybius* occurs alone, as analyzed by Dr. Frankland, contained $1\frac{1}{2}$ per cent. of nitrogenous organic matter.

2. "Notes on the Geology of China, with more especial reference to the provinces of the Lower Yungtsi." By Thomas W. Kingsmill, Esq.

The sedimentary deposits of the south of China were described as commencing at the base with a series of coarse grits and sandstones, having a thickness of about 12,000 feet, and overlain conformably by limestones and shales (with coal in the lower part), attaining a thickness of between 6000 and 8000 feet. The whole of these rocks were described by the author as the "Tung-ting Series." In the Nanking district this formation is succeeded by sandstones, grits, and conglomerates, which the author has grouped together under the name of the "Chung-shan Series." Its uppermost member contains beds of coal, and possesses an unknown thickness; but the remaining beds are together about 2400 feet thick. Mr. Kingsmill described in detail the geological relations and geographical extension of these rock-masses; he then gave a sketch of the superficial deposits, which occupy an important position in the geology of China, and from the older of which Mammalian bones and teeth have been obtained; and he concluded by stating that he had been uniformly unsuccessful in his frequent searches for traces of glacial action.

January 13th, 1869.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—

1. "On *Hyperodapedon*." By Prof. T. H. Huxley, LL.D., F.R.S., Pres. G.S.

The author described the characters of the genus *Hyperodapedon*, dwelling especially upon those presented by the head and dentition. The head presents indications of a bone forming a second zygomatic arch on each side; the upper jaw is produced and bent downwards, forming a strong beak; and the lower jaw is produced on each side of the symphysis into a pointed process, between which the decurved beak of the upper jaw is received. The maxillary and palatine teeth are arranged in rows, and present some resemblance to the large nails in the sole of a boot; they are inserted on each side of the upper jaw upon the sloping sides of a deep groove, and are worn down and polished by the action of the mandibular teeth, which form a continuous and very close single series along the upper edge of the mandible. The author remarked upon this peculiarity of arrangement, which, he said, enables the teeth of *Hyperodapedon* to be recognized wherever they may occur. The vertebræ have their centra slightly concave at each extremity. The other known parts of the

skeleton described by the author were the ribs, scapula, coracoid, and part of the humerus, the pelvis, femur, and proximal ends of the tibia and fibula, and the abdominal false ribs, which are largely developed in this Reptile.

The author declared the affinities of *Hyperodapedon* to be decidedly Lacertilian. Its nearest fossil ally is the Triassic genus *Rhynchosaurus*, and in the present day its type of structure is most closely reproduced by the singular genus *Sphenodon* (= *Hatteria*) of New Zealand. In its habits *Hyperodapedon* was probably terrestrial, or perhaps fluviatile; in Warwickshire and India it is associated with *Labyrinthodonts*. The remains hitherto met with do not justify the formation of more than one species, *Hyperodapedon Gordoni*; and the genus ranges from Britain to Central India, indicating a great extent of dry land during the period to which it belongs.

Specimens of *Hyperodapedon* from the Trias of Warwickshire, collected many years ago by Dr. Lloyd, were exhibited; but in discussing the question whether *Hyperodapedon* is to be regarded as determining the Triassic age of any rock in which it may be found, the author referred to the fact that Crocodiles bridge over the whole interval between the Mesozoic and existing conditions, and *Beryx* in like manner connects the Cretaceous with our present fish-fauna. As *Hyperodapedon* is at least as nearly allied to the existing genus *Sphenodon* (= *Hatteria*) as it is to the Triassic *Rhynchosaurus*, the author inquires why may it not have inhabited the dry land of the Permian, Carboniferous, or Devonian period? Carrying the idea thus raised still further, he indicates, from certain relations between the Reptilian faunæ of Europe, S. Africa, and India at the period when *Hyperodapedon* lived in the first and third of these localities, not only that there must then have been a vast extent of continental land, but that this may have persisted with but little change in the nature of its inhabitants, while the fauna of the neighbouring seas underwent great alterations. He remarked that our geological chronology rested too much upon a marine foundation, and that such a persistence of dry land as was now suggested by him was not only possible, but, in the present case, probable. He suggested the use of Conybeare's term "Poikilitic" for the series of deposits containing the remains of terrestrial and fluviatile plants and animals and corresponding with the marine beds denominated Permian and Triassic. Finally, the author remarked upon the important light thrown upon the question of the geographical distribution of animals as affected by the discovery of these Reptiles and other recently detected fossils, and upon the interest attaching to them from their high grade of development. The five great classes of Vertebrata were represented during the "poikilitic" epoch by species so high in the scale that we can hardly doubt their having been preceded by other forms, so that some of us may hope to see the fossil remains of a Siluriam mammal.

Sir R. I. Murchison argued in favour of the overwhelming importance of palæontological evidence, and maintained that *Hypero-*

dapedon was Triassic. He objected to the use of the term "poikilitic," which was merely indicative of the spotted character of the beds, and protested against the mingling of the Permian and Triassic series.

2. "On the Locality of a new Specimen of *Hyperodapedon* on the South Coast of Devon." By W. Whitaker, Esq., F.G.S.

The author described the section presented by the South Devon coast westward from the great landslip at Dowlands. The cliffs here show Rhætic beds passing down into Red Marls of Upper Triassic age, which have greenish layers among them, favouring the view that the Rhætic beds might as well be classed with the Trias as with the Lias. Below these beds are Red Marls and Sandstones; and at Budleigh Salterton a bed of quartzite pebbles occurs. West of the Exe the cliffs are of sandstone with layers of breccia; and beyond Dawlish the breccia gradually predominates, until towards Teignmouth the cliffs are almost wholly formed of it. This breccia forms the base of the New Red of Devonshire. The thickness of the whole series is several thousand feet; Mr. Pengelly estimates that it may be four miles or more. The jaw of *Hyperodapedon* referred to by Professor Huxley was found in the sandstone on the left bank of the Otter, immediately above the Budleigh-Salterton pebble-bed, in the lower part of the uppermost bed of sandstone, which, with the other sandstones and marl-beds, the author regarded as belonging to the Keuper. He referred to the opinions of Mr. Pengelly and Mr. Ormerod, and suggested that the breccias might possibly be of Permian age.

Sir Charles Lyell, referring to the occurrence of *Hyperodapedon* with *Stagonolepis* and *Telerpeton* in the uppermost sandstones of Elgin, remarked that he came to the conclusion in 1859 that these beds were Triassic, and that Mr. Symonds had in that year stated them to be the equivalents of the *Rhynchosaurus*-sandstones of Shropshire.

Professor Ramsay regarded the Red Marls and Sandstones described by Mr. Whitaker as Keuper, and the lower members of his section as of Permian age. He confirmed Prof. Huxley's views as to the existence of a great extent of continental land at the epoch when *Hyperodapedon* and the Reptiles associated with it were in existence, and remarked that these Reptiles inhabited the shores of the great salt lakes of the Triassic land. He objected to the use of the term "poikilitic," and remarked that if the idea embodied by Prof. Huxley under it were to be accepted, it would have to be extended to all terrestrial deposits from the Silurian period to the present day.

Dr. Günther referred to his description of *Sphenodon* (=*Hatteria*), and remarked that in that genus there are uncinate processes on the ribs, as in Birds, which do not exist in *Hyperodapedon*. He remarked upon the resemblance of the beak in the latter to that of the Tortoises, especially *Trionyx*, and suggested that the jaws might have had a horny covering.

Dr. MERYON inquired as to the implantation of the teeth in the jaws of *Hyperodapedon,* and suggested that the position and direction of the orbits were not accordant with terrestrial habits, and also that the absence of processes on the ribs indicated a flexibility of the body consistent with a fluviatile mode of life.

Prof. HUXLEY showed that no conclusion could be drawn from the want of processes on the ribs or the position of the orbits as to the habits of the animal, and remarked that the processes in *Sphenodon* were not anchylosed to the ribs; he considered it possible, but not probable, that the jaws had a horny covering. He stated that in using the term "poikilitic," he was desirous of indicating that, while several marine formations with changing forms of life succeeded each other, the terrestrial fauna may, in certain cases, have been continuous. He believed that terrestrial forms were at least as persistent as marine.

Dr. CARRUTHERS remarked that the Permian vegetation showed mesozoic affinities, and in fact that the commencement of the Mesozoic flora was to be sought in the Permian.

January 27th, 1869.—J. Gwyn Jeffreys, Esq., F.R.S., Treasurer, in the Chair.

The following communications were read:—

1. "Notes on Graptolites and allied Fossils occurring in Ireland." By W. H. Baily, F.G.S. [First Paper.]

After remarking that the Graptolites are now generally regarded as belonging to the class Hydrozoa, the author detailed the various localities in the south of Ireland in which they had been found, and indicated the species occurring in each place. The localities are situated in the counties of Waterford, Wexford, Clare, and Tipperary; and the species are as follows:—

Didymograpsus sextans, *Hall.*
—— elegans, *Carr.* (=D. flaccidus, *Hall?*, *Nich.*).
—— caduceus, *Salt.*
—— Forchhammeri.
Graptolithus (sagittarius) Hisingeri, *Carr.*
—— Sedgwicki.
—— tenuis.
—— priodon.
Cladograpsus gracilis, *Hall.*
Diplograpsus pristis, *His.*
—— mucronatus.
—— teretiusculus.
—— dentatus, *Brongn.*
Climacograpsus bicornis, *Hall.*
Dicranograpsus ramosus, *Hall.*
Cyrtograpsus gracilis, *Hall.*
—— hamatus, *Baily.*

The most widely distributed of all is *Diplograpsus pristis*, to which the author thinks *D. mucronatus* and *dentatus* probably belong. The fossils described by the author as *Theca cometoides* may probably be the gonothecæ of *D. pristis,* as had been suggested by Mr. Carruthers.

2. "Notice of Plant-remains from beds interstratified with the Basalt in the county of Antrim." By W. H. Baily, Esq., F.G.S.

The deposit referred to by the author was discovered by the late M. G. V. Du Noyer in cuttings of the Northern Railway of Ireland near Antrim; it consists of a layer from 4 to 8 inches in thickness, separated by a conglomerate bed of 10 or 12 feet from the underlying basalt, and by earthy beds of about equal thickness from the

superficial basaltic layer. The remains are imbedded in a Red Clay, and associated with hæmatitic iron ore.

The author regarded a large cone as that of a true *Pinus*, and branches of another coniferous tree as belonging to a *Sequoia* nearly allied to *S. Sternbergi*, Heer; of this a small imbricated cone might possibly be the fruit. Other fragments of Coniferæ seem to belong to *Cupressites* or *Taxites*. The fossils consist chiefly of leaves of true Dicotyledonous plants. The author identified some of these with species of *Rhamnites*, *Olea*, *Fagus*, and *Quercus*. Leaves of endogenous plants, such as Sedges and Grasses, occur not unfrequently. A large mass of fossil wood of dicotyledonous structure was obtained from the hæmatitic conglomerate. *Carpolithes* are also found. The vegetable remains are accompanied by a few elytra of Beetles.

The author remarked that these remains seem to differ as a group from those of the island of Mull. Their alliance appears to be with Mid-European forms, and they are certainly of Upper-Tertiary age, probably Miocene.

3. "Remarks upon the Basalt Dykes of the Mainland of India opposite to the Islands of Bombay and Salsette." By G. T. Clark, Esq., F.G.S.

The author described the general features of the country referred to, and stated that the dykes which traverse it vary from 1 or 2 to 100 or 150 feet in width, and often extend many miles. They are all basaltic, with a tendency to prismatic structure, but never columnar. The adjacent Trap is but little modified, only somewhat hardened, so as to resist weathering; by this means long, narrow ridges, more or less deeply furrowed above by the weathering of the basalt dyke itself, are produced. The general direction of the dykes is parallel to the lines of volcanic vents; those near the main axis of the Concan lying N. and S., and those near the subordinate axis in the Malseji valley, about E.N.E. and W.S.W. They run nearly straight, and have their faces usually parallel, but sometimes swell out or contract, or include a rider. The author considered that these dykes were formed probably during the latest periods of volcanic action in Western India. They probably belong, in his opini n, to two periods, as dykes of different grain frequently intersect each other. The dykes running N.E. and S.W. often traverse and slightly dislocate those lying more N. and S., and are probably of later date.

4. "On Auriferous Rocks in South-eastern Africa." By Dr. Sutherland.

Fourteen years ago the author expressed the opinion that gold would be found in the metamorphic rocks of Natal. A few months since Mr. Parsons found this metal by washing the iron-sand of some of the southern rivers of the colony. The gold is in microscopic rounded grains. Dr. Sutherland considers that the gold is diffused as minute particles in the granite and gneiss underlying the Silurian rocks of South Africa.

These old gneissic rocks are very much contorted, include ex-

tensive veins and lenticular masses of quartz, and are traversed by basalts. The Silurian strata, resting unconformably on the gneiss, have been invaded by igneous matter (which is never granitic), and, though generally horizontal, are frequently flexuous, and in some places greatly faulted, to the extent of even 1000 feet, together with the gneissic rocks beneath. These latter have been deeply eroded by the rivers, frequently to the depth of 500-1000 feet, and even of 3000 feet in some valleys; and in the alluvia of these valleys the gold occurs. The valleys have sometimes evidently commenced in great displacements, forming "valleys of elevation," on which the denuding agency has been operating ever since.

In certain mountains in the basin of the St. John's River, Natal, dioritic rock traverses the secondary strata; and along the line of contact it contains copper-ores with 100 grains of gold to the ton.

Mr. David Forbes was glad to find that Dr. Sutherland corroborated his views as to the occurrence of gold in two ways:—

1. In auriferous granite, as in Wicklow and elsewhere.

2. In eruptive diorite, a basic rock without free quartz, and certainly of postoolitic date, almost always accompanied by copper veins. Most Californian alluvial deposits of gold were derived from this class of rocks.

In constructing some of the railways of South America the granite was found to be so soft, from decomposition, that it could be cut with the pick and spade; and this softened granite, when washed, produced gold.

Prof. T. Rupert Jones considered that, by means of Dr. Sutherland's communication, the Laurentian and Silurian rocks were now, for the first time, to be recognized as existing beneath the *Dicynodon*-rocks of the Natal ridge.

XXIX. *Intelligence and Miscellaneous Articles.*

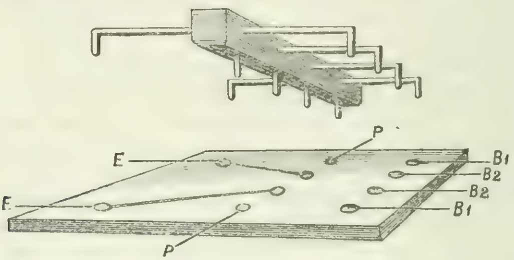
NOTE ON ELECTROLYTIC POLARIZATION. BY PROFESSOR TAIT.

I HAD just obtained one of Sir W. Thomson's most recent forms of quadrant electrometer, and it occurred to me that *this* must be the proper instrument for determining polarization, as its indications are not affected by electric resistance, and give directly (that is, without assuming the truth of Ohm's law for reverse electromotive forces, and the consequent necessary determinations of resistance) the quantities required. The method employed by Wheatstone, Poggendorff, Buff, and others assumes that the whole electromotive force in the circuit is the algebraic sum of those of the decomposing battery and of the electrodes—an assumption whose truth some may consider to require proof, and which it is certainly useful to verify by an independent process. Again, after the decomposing action has ceased, the resistance of the films (of gas or oxide) which are deposited on the electrodes may change in value. That neither of

these circumstances produces any marked effect is, however, amply proved by the numbers which follow, which, though given only as first approximations, are within the limits of difference of the results given (from galvanometric determinations) by former experimenters.

As the polarization in most cases diminishes with very great rapidity from the instant of breaking contact with the decomposing battery, and as (for this and other reasons) the mode of measurement by the first swing of the index-needle of the electrometer is not deserving of much confidence, it was necessary to devise a process by which the electrometer could be charged at leisure up to any desired potential, and then, for an instant only, placed in connexion with the electrodes. The apparatus I employed bears a certain analogy to the *Wippe* of Poggendorff, but differs from it in some essential particulars, both of construction and mode of working.

In a plate of vulcanite, or other good insulator, ten holes are cut as below, and filled with mercury. Those marked E are connected



with pairs of opposite quadrants of the electrometer, P with the electrodes, B_1 with the decomposing battery, and B_2 with the auxiliary (or charging) battery. Also metallic connexion, as indicated in the sketch, is permanently established between the two central holes and the holes connected with the electrometer.

The rocker consists of four wires, supported on an insulating bar of vulcanite, the two outermost having three points, the middle one longer than the others, and the two inner being similar, but wanting one of the extremities. When the four middle stems dip vertically into the four central mercury-cups, the other stems do not reach the mercury in any of the other six cups. If the instrument be inclined to the right the four prongs enter the holes to the right, thus simultaneously connecting the electrodes with the decomposing battery, and the electrometer with the charging battery. When the instrument inclines to the left, the electrodes are shunted from the decomposing battery on to the electrometer, the latter having just before, by the same action, been cut off from the charging battery, and thus left charged.

The *modus operandi* is simply this:—Leave the rocker leaning to the right by its own gravity, decomposition and polarization going on; adjust the wires B_2 to different points in a wet string (or a narrow canal of water) closing the circuit of the charging battery;

work the rocker quickly to the left, and allow it instantly to fall back again—a process which need not occupy more than a small fraction of a second, yet which must not be performed too quickly, on account of the inertia (small as it is) of the needle and mirror of the electrometer. If the deflection of the electrometer be suddenly increased or diminished by this action, slide one of the wires B_2 along the wet string, a little further from or nearer to the other, and rock again,—continuing this process till a charge is found which leaves the electrometer at rest when the rocking to and fro is performed. Reverse a commutator attached to the wires E, and repeat the operation. The difference of the scale-readings in these two cases gives a number proportional to the electromotive force of the polarized plates—(I say *difference*, because the scales commonly used with Sir W. Thomson's instruments are, to avoid confusion, graduated from one end to the other, as they ought to be, instead of being graduated opposite ways from the middle). To enable this measure to be reduced to absolute units, a normal Daniell's cell was applied at intervals, during each day's work, directly to the electrodes of the electrometer, then reversed; and the difference of the readings was tabulated as representing its electromotive force.

In the other experiments I used a plate of gutta percha in which the ten holes were bored, but for a time discontinued its use on suspecting that it sometimes led to irregular working of the apparatus by imperfect insulation. The cups were then *separately* mounted on insulators 3 inches high; but this was not found to be an improvement of any consequence, and the holes are now made in a small, but thick, plate of vulcanite.

In this note the numbers presented must be looked upon only as first approximations; but the apparatus has now been carefully constructed by an instrument-maker, and Mr. Dewar has begun an elaborate series of experiments with it, from which valuable results may soon be expected. In the trials which have as yet been made we employed a temporary apparatus, rudely built up of wires, sealing-wax, and gutta percha. We have rather been endeavouring to determine whether the process, complicated as it is by the inertia of the moveable part of the electrometer, the quickness with which the rocking can be conducted, and the rate at which the polarization begins to diminish as soon as the polarized plates are detached from the decomposing battery, is capable of being made to give good results, than in actually attempting to get such. So far as I can yet see, the first of these complications is alone likely to cause any serious embarrassment; and should such be the case, which I do not anticipate, a form of experiment a little more laborious than that above described, and which I have already once or twice tried, seems to be well adapted to meet it.

The following are, for the most part, means of a great number of determinations. The electrolyte was usually dilute commercial sulphuric acid, 1 part acid to 10 of water; and to the lead and other impurities it was found to contain we may ascribe the fact that the results were not very accordant from day to day, so that it was not

easy to decide how to take the means. Mr. Dewar is now working with substances chemically pure, and obtains much more constant results.

The unit employed is the electromotive force of an ordinary Daniell's cell. The Grove's cells used in the electrolysis had (very constantly) an electromotive force about 1·74 as great.

I. *Freshly-burned Platinum Plates.*

Number of Grove's cells in decomposing battery	1	2	3	4	8
Resulting polarization ..	1·64	1·98	2·01	2·12	2·30

II. *Platinum +, Palladium −.*

Cells	1	2	4
Polarization	1·50	1·82	1·85

III. *Palladium +, Platinum −.*

Cells	1	2	4
Polarization	1·60	1·92	1·91 (?)

IV. *With Three Cells.*

	Platinum +, Iron −.	Platinum −, Iron +.	Iron plates.
Polarization..	2·16	0·0	0·0

V. *Aluminium Plates.*

Cells	1	2	3	4	6
Polarization .	1·09	2·17	2·44 (?)	4·01	5·20

The last results are very remarkable, showing, as they do, from aluminium electrodes a reverse electromotive force of more than five Daniell's when six Grove's are in circuit. The polarization alters so rapidly during the electrolysis (in the case of aluminium) that I cannot be certain that the numbers above given represent fully the maximum effect. Various other combinations have been tried, but are being repeated by Mr. Dewar.—*From the Proceedings of the Royal Society of Edinburgh,* Session 1868–69.

SPECTRUM OF THE AURORA BOREALIS.

BY J. A. ÅNGSTRÖM.

From the time when Franklin made his remarkable experiments on lightning, to the present time, a complete parallelism has been shown to exist between the actions of the forces of nature and those of frictional electricity; and hence it might have been expected that the spectrum of lightning would be like the spectrum produced by the ordinary electrical discharge. This has also been fully proved by M. Kundt's observations. As, moreover, the aurora borealis and terrestrial magnetism are so intimately connected that the occurrence of the former phenomenon is always connected with disturbing actions on the magnetic needle, it might be assumed that the northern light is nothing more than an electrical luminosity, such as is produced in the electrical egg in rarefied air.

This, however, is not the case. In the winter of 1868–69 I was several times able to observe the spectrum of the luminous arc which surrounds the dark segment, and is never wanting in faint auroræ.

The light was almost monochromatic, and consisted of a *single bright line*, which was on the left of the well-known group of lines of calcium. By measuring its distance from this group I determined the wave-length of the line, and found it

$$\lambda = 5567.$$

Besides this line, the intensity of which is relatively very great, I observed, after the slit had been widened, traces of three very faint bands which extended nearly as far as F. Only once, when the luminous arc was much agitated, owing to undulations which altered its shape, did I see the regions in question momentarily illuminated by some faint spectrum-lines; yet, from the feeble intensity of these rays, we may still say that the light of the luminous arc is almost monochromatic.

One circumstance imparts to this observation of the spectrum of the aurora borealis a far greater, I may almost say cosmical, interest. In March of 1867 I observed for a whole week the same line in the zodiacal light, which at that time displayed an extraordinary intensity. Finally, on a starlight night, when the whole sky was in some degree phosphorescent, I found traces even in the faint light which proceeded from all parts of the heavens.

It is a remarkable fact that the line in question does not coincide with any of the known lines in the spectra of simple or of compound gases—at any rate, as far as I have investigated them.

From what has just been said it follows that an intense northern light, such as can be observed within the polar circle, will probably give a more complex spectrum than that which I have observed. If this be the case, we may also hope that in the future we shall be able to explain more easily the origin of the lines found and the nature of the phenomenon itself. But since I cannot at present give this explanation I intend to revert to it on a future occasion.—Poggendorff's *Annalen*, May 1869.

ON THE THERMAL ENERGY OF MOLECULAR VORTICES. BY W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS. LOND. & EDINB. ETC.*

In a previous paper, presented to the Royal Society of Edinburgh in December 1849, and read on the 5th of February 1850 (Transactions, vol. xx.), the author deduced the principles of thermodynamics, and various properties of elastic fluids, from the hypothesis of molecular vortices, under certain special suppositions as to the figure and arrangement of the vortices, and as to the properties of the matter which moves in them. In subsequent papers he showed how the hypothesis might be simplified by dispensing with some of the special suppositions. In the present paper he makes further progress in the same direction, and shows how the general equation of thermodynamics and other propositions are deduced from the hypothesis of molecular vortices when freed from all special suppositions as to the figure and arrangement of the vortices, and the properties of the matter that moves in them, and reduced simply to the following form—*that thermometric heat consists in a motion of the particles of*

* Communicated by the Author, having been read before the Royal Society of Edinburgh, May 31, 1869.

bodies in circulating streams with a velocity either constant or fluctuating periodically. This, of course, implies that the forces acting amongst those particles are capable of transmitting that motion.

The principal conclusions arrived at are the following:—

(1) In a substance in which the action of the vortices is isotropic, the inteusity of the centrifugal pressure per unit of area is *two-thirds* of the energy due to the steady circulation in a unit of volume. The centrifugal pressure is the pressure exerted by the substance in the perfectly gaseous state.

(2*) If there be substances in which the action of the vortices is not isotropic, then in such substances the proportion already stated applies to the mean of the intensities of the centrifugal pressures in any three orthogonal directions.

(3*) The proportion which the whole energy of the vortices, including that of the periodic disturbances, bears to the energy of the steady circulation alone may be constant or variable.

(4) Absolute temperature is proportional to the energy of the steady circulation in unity of mass, and to the specific volume in the perfectly gaseous state.

(5) In substances which are nearly in the perfectly gaseous state, experiment shows the proportion in which the whole energy exceeds that of the steady circulation to be sensibly constant; and its value may be found by computing in what proportion the dynamical value of the specific heat at constant volume exceeds once and a half the quotient found by dividing the product of the pressure and volume by the absolute temperature. *The following are examples:—air, 1·634; nitrogen, 1·630; oxygen, 1·667; hydrogen, 1·614; steam-gas, 2·242.

(6) The known general equation of thermodynamics is deduced from the hypothesis of molecular vortices*, freed from the special suppositions made in the paper of 1849–50.

The new conclusions obtained in the present paper are marked *, Those not so marked were arrived at in the paper of 1849–50.

[The general equation of thermodynamics is here stated for convenience:—Let $d\mathrm{Q}$ be the thermal energy which must be given to unity of mass of a given substance in order to produce a given indefinitely small change in its temperature and dimensions; then

$$d\mathrm{Q}=\tau d\,.\,\phi\,;$$

in which τ is the absolute temperature, and ϕ the thermodynamic function. The value of that function is

$$\phi=\mathrm{J}c\,\text{hyp. log}\,\tau+\chi(\tau)+\frac{d\mathrm{U}}{d\tau},$$

$\mathrm{J}c$ being the dynamical value of the real specific heat, U the potential energy of the elasticity of the body at constant temperature, and $\chi(\tau)$ a function of the absolute temperature, which is null or inappreciable in a substance capable, at that temperature, of approximating indefinitely to the perfectly gaseous state, and is included in the formula in order to provide for the possibility, suggested by Clausius, that there may be substances which have not that property at all temperatures.]

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XXX. *On the Spectra of Carbon. By* W. M. Watts, *D.Sc., Physical-Science Master in the Manchester Grammar School**.

[With a Plate.]

ALTHOUGH considerable progress has been made in spectrum-analysis since its first principles were enunciated by Bunsen and Kirchhoff, we still seem to be in considerable uncertainty as to the changes in the spectrum of an element which it is possible to bring about by altering the conditions under which it is produced. The interesting investigations of Plücker and Hittorf and of Wüllner have shown that it is possible for an element to have more than one spectrum; and these totally different spectra have been supposed to belong to different allotropic modifications, apparently on the supposition that changes of temperature produce changes in the spectrum consisting merely in the addition of new lines. The following observations, in which four different spectra are described as belonging to the element carbon, are offered as contributions to our knowledge of this subject.

The principal previous investigations on the spectra of the carbon-compounds, to some of which reference is afterwards made, are comprised in the following list:—

Swan, Edinb. Phil. Trans. vol. xxi. p. 411 (1856).

Attfield, Phil. Trans. 1862, p. 221.

Plücker, Pogg. *Ann.* vol. cvii. p. 497.

Dibbits, Pogg. *Ann.* vol. cxxii. p. 499, and *De Spectraal Analyse.*

* Communicated by the Author.

Plücker and Hittorf, Phil. Trans. 1865, p. 1.

Morren, *Ann. de Chim. et de Phys.* 1865, vol. iv. p. 305.

Lielegg, Phil. Mag. S. 4. vol. xxxiv. p. 302; vol. xxxvii. p. 208.

Deville, *Leçons sur la Dissociation,* and Phil. Mag. S. 4. vol. xxxvii. p. 111.

Wüllner, Phil. Mag. S. 4. vol. xxxvii. p. 405.

Frankland, Proc. Roy. Soc. vol. xvi. p. 419.

I select as the typical form of the first carbon-spectrum that obtained when olefiant gas and oxygen are burnt together in an oxyhydrogen blowpipe-jet. The flame thus obtained exhibits a central cone of intense green, which, examined by the spectroscope, gives the spectrum first obtained by Swan, and ascribed by Attfield to the vapour of carbon. The spectrum which is drawn, Plate I. fig. 1 *a*, is one of the most beautiful which can be imagined, and consists of five groups of lines—α in the red, γ in the greenish yellow, δ brilliant emerald-green, ϵ in the blue, and f violet.

Group α * contains five lines, of which the third is the brightest. γ contains seven, of which the least refracted is the brightest, and each succeeding line is less brilliant than the one before; so that the group rises sharply out of darkness on the left, and fades gradually away on the right. The group δ, which contains four lines, presents the same gradation of intensity: ϵ contains four lines of nearly equal intensity, the fourth being double; and f consists of a broad band, then a fine bright line, and then a band fading away on the most refracted side. When the spectrum is obtained very brightly, there may be observed in addition six very fine bright lines of equal intensity, which gave the readings 86, 87·5, 89, 91, 93, 95. The band 128–133 is also seen to be shaded by a large number of nearly equidistant fine dark lines; and the least refrangible band of the group f (121–126) is resolved into lines.

This spectrum may be obtained from the flame of any hydrocarbon, though in many cases, owing to the faintness of the spectrum, only some of the groups can be recognized. In the flame of an ordinary Bunsen burner δ and ϵ are easily seen, γ and f are much fainter, and the red group cannot be detected.

This spectrum is proved to be that of carbon, inasmuch as it can be obtained alike from compounds of carbon with hydrogen, with nitrogen, with oxygen, with sulphur, and with chlorine. I have obtained it, namely, from each of the following com-

* This is the group described as new by Professor Lielegg, Phil. Mag. March 1869. It is true, as he notes, that Dibbits strangely omits it, and that Plücker and Hittorf give only three lines; but the group of five lines is given by Morren, and they are distinctly figured in the drawing to my paper on the Bessemer-spectrum in the Philosophical Magazine for December 1867.

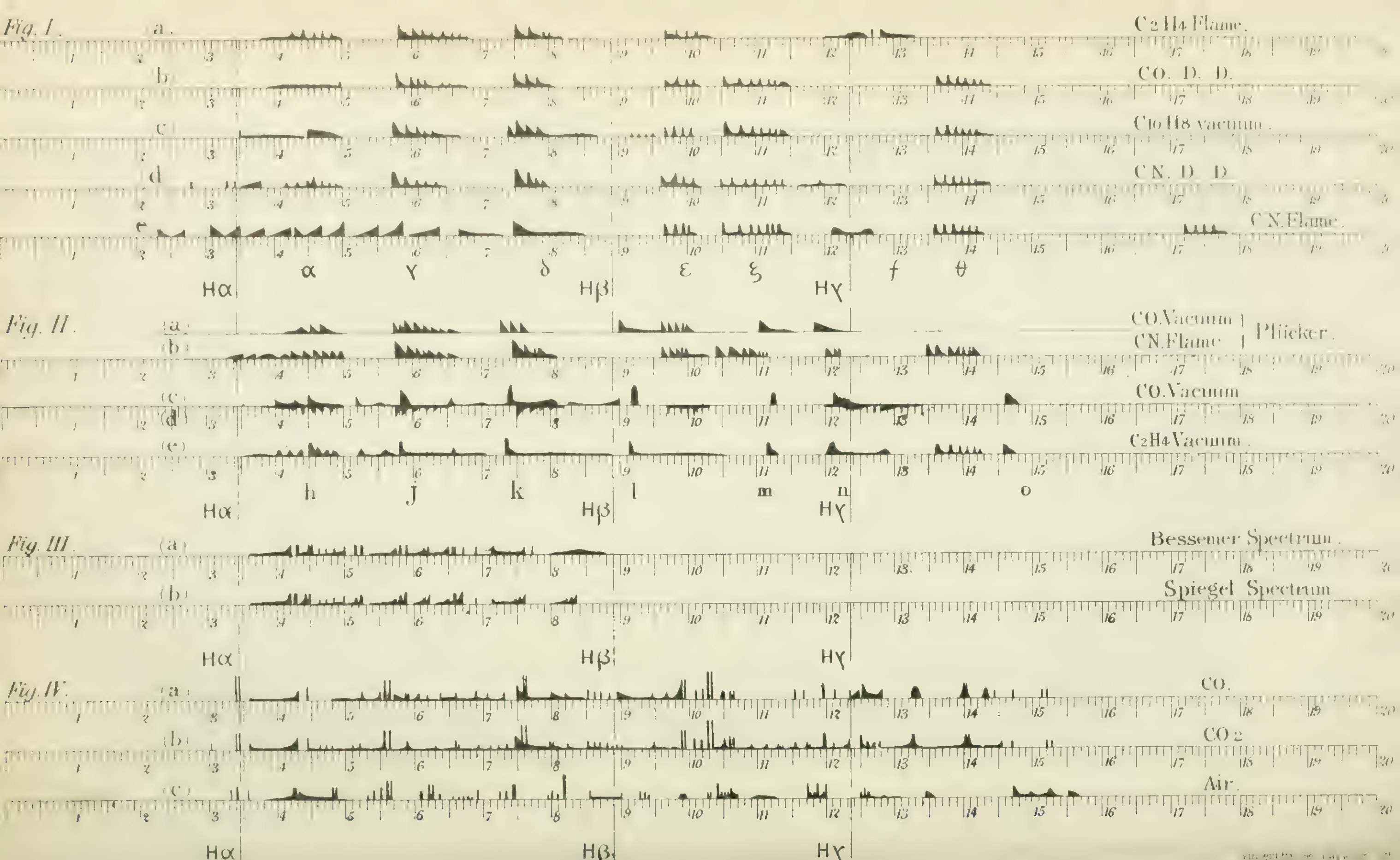


Fig. I.
a. C2 H4 Flame.
b. CO. D. D.
c. C10 H8 vacuum.
d. CN. D. D.
e. CN. Flame.
Hα α γ δ Hβ ε ξ Hγ f θ
Fig. II.
(a) CO. Vacuum
(b) CN. Flame
Plücker.
(c) (d) CO. Vacuum
(e) C2H4 Vacuum.
Hα h j k Hβ l m n Hγ o
Fig. III.
(a) Bessemer Spectrum.
(b) Spiegel Spectrum
Hα Hβ Hγ
Fig. IV.
(a) CO.
(b) CO2
(c) Air.
Hα Hβ Hγ

pounds:—olefiant gas, cyanogen, carbonic oxide, naphthalin, carbonic disulphide, carbonic tetrachloride, amylic alcohol, and marsh-gas.

It may be obtained from olefiant gas either by burning the gas with oxygen, as already described, or by taking the spark of an induction-coil in the gas at the ordinary pressure. In the latter case, however, carbon is rapidly set free and the spectrum becomes continuous. The electric discharge in olefiant gas under diminished pressure gives a different spectrum, which will be afterwards described.

The spectrum obtained from cyanogen varies with the mode of production. The flame of cyanogen in oxygen exhibits γ, δ, and ϵ. The red group is replaced by a series of bands which show an opposite character to the rest of the spectrum, inasmuch as each band is brightest at the most refracted edge. If cyanogen be burnt in air instead of in oxygen these bands are more numerous, extending nearly to δ, and replacing γ, which is then not to be seen*. Instead of the group f we have two very brilliant groups of lines—ζ, which includes seven lines (105–113), and θ, which is composed of six lines (136–142). Fig. 1 *e* is a reduction of Dibbits's drawing of the spectrum of cyanogen burning in air (*De Spectraal Analyse*), and agrees well with my own observations.

If the cyanogen, instead of being burnt, be rendered incandescent by the discharge of an induction-coil in the gas at the ordinary pressure, a spectrum is obtained which contains γ, δ, ϵ, ζ, and θ, but which does not exhibit f. The red group α may be obtained precisely the same as from the olefiant-gas flame; but when the intensity of the spark is increased a different aspect comes out, which is represented in the Plate, fig. 1 *d*.

Precisely the same spectrum is obtained from a Geissler's tube enclosing cyanogen of a few millimetres pressure. The spectrum consists of α, γ, δ, ϵ, ζ, and θ.

When a Leyden jar is included in the circuit, the relative intensity of the lines is altered, but the spectrum is essentially the same, with the addition of the nitrogen-lines obtained from the spark in air.

The flame of carbonic oxide gives only a continuous spectrum; but if the induced spark be taken in the gas at the atmospheric pressure, we obtain again the carbon-spectrum, comprising sometimes γ, δ, ϵ, and f, and sometimes γ, δ, ϵ, ζ, and θ. The red end is too faint to determine. The replacement of the group f by ζ and θ is very curious, but I have been unable to

* These bands are thus obtained more completely developed at the lower temperature of the flame in air, and are doubtless due to the compound cyanogen itself.

determine the conditions on which the presence of one or the other of these groups depends. A touch of the contact-breaker will sometimes cause f to disappear and be immediately replaced by the other two groups. The change of temperature (if it be so) thus caused is not attended, then, simply by the addition of new lines, but causes the disappearance of one group and its replacement by two other quite different groups of lines. When a Leyden jar is included in the secondary circuit, no trace of the carbon-lines is obtained if the jar be large enough, but instead a brilliant spectrum, which is described afterwards as the fourth carbon-spectrum and is represented in fig. 4a. I have employed, instead of a Leyden jar, *a graduating condenser* consisting of two opposed disks of metal, the distance between which could be varied at pleasure. When the plates are separated, the condensation is so feeble that the spark in carbonic oxide shows the carbon-spectrum only; but as the plates are gradually approximated, the fourth carbon-spectrum appears gradually replacing the old spectrum and finally completely extinguishing it. The blue band f is the first to disappear, and is replaced by the group 123–133 of fig. 4, and the conspicuous line 76 of fig. 4 appears nearly bisecting the interval between the first and second lines of the group δ.

When the density of the carbonic oxide is increased while the spark (without condenser) passes through it, the gas is more rapidly decomposed, the spark becomes more luminous, and the spectrum more nearly continuous. At two atmospheres' pressure the spectrum obtained is the carbon-spectrum, consisting of γ, δ, ϵ, ζ, and θ (the red end probably contains α), ϵ, ζ, and θ being very brilliant. Increase of pressure up to about ten atmospheres only produces the effect of filling up the intermediate spaces with white light.

The spectrum, including the groups ζ and θ, is also obtainable from compounds of carbon with hydrogen. A Geissler's tube enclosing naphthalin gives a splendid carbon-spectrum, in which the groups ζ and θ are especially brilliant. They are therefore abundantly proved to be produced by carbon itself.

By passing the spark through the vapour of carbonic disulphide, there can be obtained at will either Plücker's sulphur-spectrum of the second order or the carbon-spectrum on a background of continuous light due to the separation of sulphur.

The spark in the vapour of carbonic tetrachloride gives either the carbon-spectrum or the chlorine-spectrum, according to circumstances.

A Geissler's tube enclosing amylic alcohol gives the carbon-spectrum, consisting of α, γ, δ, ϵ, and f.

A Geissler's tube enclosing marsh-gas gives γ, δ, ζ, and θ, but the group ϵ is not observed. This spectrum contains also a line at 74, which may belong to the second carbon-spectrum.

Carbonic oxide has been stated to yield the ordinary carbon-spectrum when the induced spark is taken in the gas at the ordinary pressure. The discharge through a Geissler's tube, however, exhibits an entirely new spectrum which contains none of the ordinary carbon-lines. That this new spectrum is also due to carbon itself is shown by the fact that it is obtained either from a vacuum-tube enclosing carbonic oxide, or from one enclosing olefiant gas*; and it becomes a question of much interest to determine upon what conditions the production of one or the other of these forms of the carbon-spectrum depends. Olefiant gas is capable of yielding either spectrum. When the discharge is passed through a tube containing olefiant gas of only a few millimetres pressure, the spectrum drawn (fig. 2*e*) is obtained, but the gas at the ordinary pressure yields the first form. In order to determine at what pressure the second spectrum displaced the first, a tube provided with platinum wires was connected with the air-pump so that it could be exhausted, and by means of a tap with a source of olefiant gas. It was also provided with a gauge-tube, by means of which the pressure could be measured. When the pressure is about 12 millims., the spark is violet and gives the carbon-spectrum of fig. 2; when the pressure of the gas was gradually increased the spark became blue; and at a pressure of about 100 millims. the spectrum changed to that of the first form. When still more gas was admitted the spark became white, and carbon was rapidly separated.

Plücker† has observed these lines of the second carbon-spectrum. In his earlier paper he describes them as lines belonging to the compound carbonic acid; but in the paper published in 1865 he represents them as belonging to carbon itself. Fig. 2, *a* & *b*, shows the observations of Plücker, reduced from the drawing to his paper in the Philosophical Transactions to the scale employed throughout this paper. Fig. 2*a* shows the spectrum obtained from spectral tubes enclosing carbonic oxide of 32 millims. pressure. A comparison of this spectrum with that of carbonic oxide (fig. 2*c*) and with that of olefiant gas (fig. 2*e*), shows that Plücker did not succeed in completely separating the two spectra. I have, however, repeatedly obtained the second spectrum alone, consisting of the bands *h*, *j*, *k*, *l*, *m*, *n*, and *o*, and exhibiting no

* This curious difference in the spectra obtained from different carbon-compounds was first noted by Dr. Roscoe, in a lecture delivered before the Royal Institution in May 1864.

† Pogg. *Ann.* vol. cvii. (1859). Phil. Trans. 1865.

trace of $\alpha, \gamma, \delta, \epsilon$. Fig. 2, *c* and *d*, shows the result of a direct comparison of the carbonic-oxide vacuum-spectrum with that of the olefiant-gas flame when the two are seen simultaneously in the spectroscope.

The carbonic-oxide vacuum-spectrum shows the lines *h, j, k, l, m, n*, and *o*. A spectrum-tube enclosing olefiant gas (or coal-gas, or a mixture of equal volumes of olefiant gas and hydrogen) gives *h, j, k, l, m, n*, and *o*, and sometimes the group θ of the first carbon-spectrum; occasionally δ is also faintly visible. Plücker* has obtained from a vacuum-tube containing carbonic disulphide, carbon *h, j, k, l, m*, and *n*.

I believe that we have a *third* form of the carbon-spectrum in that obtained from the Bessemer-flame, which I described in a paper published in this Magazine for December 1867. Professor Lielegg† regards the Bessemer-spectrum as that of carbonic oxide. It is, however, impossible to obtain it either from the flame of carbonic oxide or from the gas rendered incandescent by electricity: in the first case a continuous spectrum only is obtained; and in the latter either the spectrum of carbon (fig. 1) or that obtained also from carbonic anhydride (fig. 4) is produced. I have always looked upon this spectrum as that of carbon itself, and have sought to obtain it from compounds of carbon with nitrogen or with hydrogen, but without success. It appears to be produced only under conditions very nearly the same as those of the Bessemer-flame itself. Thus I have observed it in one or two furnace-flames in which a very high temperature is produced. The flame of carbonic oxide in an ordinary melting-cupola gives a very brilliant continuous spectrum, but exhibits only the sodium-line. In the working of a blast-furnace it is usual, after the iron has been run, to turn on the blast so as to help the iron out. This produces a large white flame from under the tymp, which exhibits a very bright continuous spectrum with the sodium- and lithium-lines brilliant, together with a faint Bessemer-spectrum. I have observed the lines of the Bessemer-spectrum also in the flame of a small furnace, used on the works at Crewe for loosening the tyres of wheels, in which coke is burnt by a blast of air; and the Bessemer-spectrum is always obtained in the combustion of coke alone in the convertor. The spectrum of the coke-flame exhibits the Bessemer-lines faintly, and the lines of sodium and lithium: the introduction of the charge of molten pig iron seems to cool down the flame, so that for two or three minutes after the commencement of the blow a continuous spectrum only is seen. As the temperature rises the sodium-

* Pogg. *Ann.* vol. cvii. p. 538.
† Phil. Mag. S. 4. vol. xxxiv. p. 302.

line first becomes visible; then the lithium-line is added, and gradually the lines of the Bessemer-spectrum, increasing in brilliancy to the end of the blow.

The spiegel-spectrum, as I have pointed out, is only the Bessemer-spectrum in which some of the lines are still further increased in brilliancy, and is doubtless due to the highest temperature of all; for we have the hot carbon of the molten spiegeleisen burnt by the intensely heated oxygen absorbed by the liquid steel. The spiegel-spectrum is occasionally identical with the ordinary Bessemer-spectrum, when, namely (as shown by the spectroscope and by the analysis of the steel), the blast has been stopped somewhat short of the proper point. The effect of an increase of temperature is thus to split up the Bessemer-spectrum into groups of lines, in each of which the brightest line is the most refrangible—an aspect which is exactly the reverse of that so noticeable in the ordinary carbon-spectrum, where each group has its strongest line on the left hand.

A *fourth* spectrum, also probably due to incandescent carbon, is obtained from the induced spark in either carbonic oxide or carbonic anhydride when a Leyden jar is included in the circuit, and is represented in fig. 4. It is one of the spectra termed by Plücker "spectra of the second order," consisting, not of bands, but of sharply defined lines, frequently in pairs. It has been already stated that the induction-spark (without condenser) gives in carbonic oxide the carbon-spectrum No. 1, and in carbonic anhydride a continuous spectrum. With a sufficiently large condenser the spectrum obtained from carbonic oxide is identical with that obtained from carbonic anhydride, as will be seen on comparing fig. 4 *a* (spectrum of carbonic oxide) with fig. 4 *b* (spectrum of carbonic anhydride). The carbonic oxide was prepared from potassium ferrocyanide and well washed with caustic potash. The spectrum obtained from air under similar conditions is subjoined for the sake of comparison. The carbon double band $\left\{\begin{matrix}55{\cdot}5\\56{\cdot}5\end{matrix}\right.$ is at first sight identical with the double band in the air-spectrum. If, however, while the spark continues to pass, the carbonic anhydride be blown out of the discharge-tube and replaced by air, it is distinctly seen that the two are not coincident. The left-hand nitrogen-line is slightly more refrangible than the left-hand carbon-line; the right-hand members are (with one prism) apparently coincident.

The continuous spectrum obtained by the discharge of an induction-coil in carbonic anhydride may be converted into this fourth carbon-spectrum, either by increasing the electric condensation as described above, or by increasing the density of the gas. Carbonic anhydride in the compression-apparatus which I have

used for experiments on gases under pressure, shows at the ordinary pressure only a faint continuous spectrum; at two atmospheres' pressure the spectrum is much brighter but still continuous; and at pressures between seven and ten atmospheres' the spark passes with difficulty, and the spectrum shows a number of bright bands which agree in position with the lines 76, 99, 103, and 106 of fig. 4 *b*. They differ in character, however, being bands instead of fine lines, thus bearing the same relation to the fine lines obtained from carbonic anhydride at the ordinary pressure as the expanded lines of hydrogen do to the fine lines obtained from a hydrogen vacuum-tube. These bands are obtained also in the spectrum of the condensed spark in the vapour of amylic alcohol.

The spectrum of the direct discharge in a tube containing hydrogen of a few millimetres tension only and a trace of methyl-oxalic ether is faint, but contains the lines *k*, *l*, *m* of the second carbon-spectrum; but when by warming the tube the ether is volatilized, the spark passes only in brilliant flashes, and the spectrum then contains lines 34, 75, 85–90, 99, 103, 106, 120, 125, and 140 of the fourth carbon-spectrum again as *bands*.

This fourth spectrum, obtained from carbonic oxide and carbonic anhydride, may either be due to carbon, or to carbonic oxide, or to carbonic anhydride. It is, of course, not the spectrum of oxygen. I believe it to be due to *carbon*; but I have not been able to obtain such complete evidence as is afforded for the spectra Nos. 1 and 2 in their production from different carbon-compounds. Thus I have not been able to obtain this fourth spectrum from a compound of carbon with hydrogen alone; the condensed spark in cyanogen at the ordinary pressure gives, however, together with the carbon-spectrum No. 1 and the nitrogen-spectrum of the second order, the lines 34, 56, 76, and 103 of the carbon-spectrum No. 4. This conclusion (that the spectrum is really due to carbon itself) seems to be supported by the fact that, when this spectrum is obtained from either carbonic oxide or carbonic anhydride, there is always a perceptible deposit of carbon; since if it were due to carbonic oxide we should not expect to have carbon deposited in either case; and if it were due to carbonic anhydride, though carbon would be set free from the carbonic oxide, there would be none from carbonic anhydride itself. It would appear that carbonic oxide is more easily decomposed than carbonic anhydride, either into carbon and carbonic anhydride, or into carbon and oxygen; so that at the low temperature of the direct discharge carbonic oxide is decomposed and gives the carbon-spectrum No. 1, while carbonic anhydride resists decomposition. If the temperature of the spark be increased either by the intercalation of a Leyden jar or by increasing the density

of the gas, the carbonic anhydride is decomposed and the new carbon-spectrum becomes visible.

If we attempt to define the conditions under which these different forms of the carbon-spectrum are produced, we are met by very considerable difficulties. The knowledge we possess of the temperature of gases ignited by the electric discharge is so small, that we cannot with any certainty compare the spectra produced in this way with those obtained from the flames of carbon-compounds. Indeed it seems by no means certain that we are right in attributing the differences obtained in electric spectra simply to the different temperature to which the gas is heated.

In comparing the spectra of fig. 1, we notice that the changes take place at the *ends* of the spectra, the central groups γ, δ, ϵ remain substantially the same. If we pass from the spectrum of the olefiant-gas flame to that of the cyanogen-flame, we find the change at the blue end of the spectrum consisting in the disappearance of the group f and its replacement by the groups ζ and θ. The group f is not absolutely proved to belong to carbon (that is, it may be caused by carbonic oxide or carbonic anhydride); but the groups ζ and θ, since they are common to carbonic oxide, cyanogen, and naphthalin, must be due to carbon, and their presence may with much probability be attributed to the higher temperature of the cyanogen-flame.

The temperatures of flames, calculated on the assumption that the total heat of combustion is expended in heating up the products of combustion, have been shown by Deville to be immensely too high. Thus, for example, the temperature of the oxyhydrogen-flame, which calculation fixes at 6880° C., is determined experimentally by Deville* to be not higher than 2500° C., and by Bunsen not higher than 2800° C. The following are the calculated temperatures of some flames, with which are compared the recent experimental results of Bunsen †:—

	Calculated.	Experimental.
Hydrogen in air	2738° C.	2024° C.
Hydrogen in oxygen . . .	6880	2844
Carbonic oxide in air . . .	2996	1997
Carbonic oxide in oxygen .	7067	3033
Cyanogen in air	3519	3297
Cyanogen in oxygen . . .	10557	
Olefiant gas in air . . .	2619	
Olefiant gas in oxygen . .	8626	

* *Leçons sur la Dissociation*, p. 281.
† Pogg. *Ann.* vol. cxxxi. p. 161.

There is another element of uncertainty which must not be forgotten. The calculated temperatures and those obtained experimentally by Bunsen are the mean temperatures of the flames, and it is quite possible for one part of a flame to be 1000° C. hotter or 1000° C. colder than the temperature given as the temperature of the flame. The blue cone of a Bunsen gas-flame, from which the carbon-spectrum is obtained, is certainly much colder than the exterior cone of the flame at the same point.

I have made several attempts to reduce the temperature of the olefiant-gas flame, but have not succeeded in altering the spectrum at all. Olefiant gas, burnt by means of oxygen in an atmosphere of hydrogen, gives the carbon-spectrum brilliantly with all the fine lines previously described; and a mixture of olefiant gas and steam burns with a colourless flame which exhibits the same spectrum.

A mixture of 2 vols. carbonic anhydride and 1 vol. olefiant gas burns with a barely luminous flame, the blue part of which gives the groups γ, δ, ϵ, and f of the carbon-spectrum. The calculated temperature of such a flame is 2016° C.; but in all probability the temperature is much less, as no allowance is made in the calculation for any refrigerating effect produced by the decomposition of the carbonic anhydride.

The fusing-point of gold is given by Deville* as 1300° C., and of platinum as 2000° C. The interior blue cone of a Bunsen-flame about 10 millims. above the jet, which is the part which yields the carbon-spectrum most plainly, is capable of melting gold, but does not melt platinum. It is incapable of fusing steel, which is fused by the outer cone at the same point; and platinum resists the flame at any point†. We may therefore probably assign to the inner blue cone a temperature of about 1500° C.

The temperature of the flame of olefiant gas and oxygen has not been determined by experiment; but it can hardly be above 2500° C., and we may therefore conclude that the groups γ, δ, ϵ are produced by incandescent carbon between the temperatures of about 1500° C. and 2500° C.‡

In order to determine the inferior limit of the groups ζ and θ, a mixture of equal volumes of carbonic anhydride and cyanogen was made; it burnt with a violet flame of small intensity, yielding the carbon-spectrum, including the group θ and the bands

* *Leçons sur la Dissociation*, p. 284.

† A fine platinum wire, which could not be fused in any part of a Bunsen-flame, was easily fused at one point in an ordinary bat-wing gas-burner.

‡ The groups γ, δ, ϵ are those observed by Huggins in the spectrum of Winnecke's comet.

of cyanogen. As the temperature calculated for the cyanogen-flame agrees closely with the experimental result, we are probably justified in accepting the calculated temperature in this case also as not very far from the truth, and may therefore conclude that θ begins to be visible about 2200° C. Platinum and steel are easily fused in the flame of cyanogen burning in air.

The temperature of a gas ignited by the electric discharge depends upon the resistance and upon the quantity of electricity transmitted in each spark; and this may be increased either by increasing the condensing surface, or by increasing the tension of the electricity at discharge. This tension depends upon the nature of the gas: thus the spark passes with great ease through hydrogen, with more resistance through carbonic oxide, carbonic anhydride or oxygen, and with extreme difficulty through cyanogen. But for one and the same gas the tension at discharge and resistance experienced are increased by increasing the density; and the heat produced thus increasing more rapidly than the quantity of matter to be heated, the temperature rises*. Hence we understand why the groups ζ and θ are added to the spectrum of the spark in carbonic oxide when the density of the gas is increased, and why the spark in the gas cyanogen, which offers so great resistance, always gives a spectrum containing ζ and θ. We have also the explanation of the fact that a vacuum-tube containing either the dense vapour of naphthalin, or the badly conducting gas cyanogen, gives always the spectrum of carbon belonging to the high temperature, although the pressure of the gas is only a few millimetres.

It is impossible to assign any temperature as the superior limit of this first form of the carbon-spectrum which shall have any meaning, or to guess with any probability at the temperature of the condensed spark. It cannot be less than 10,000° C.; but the temperature calculated for the flame of cyanogen in oxygen (without doubt the hottest flame known) can hardly be trusted. The carbon-spectrum No. 1 may then roughly be said to be due to incandescent carbon above 10,000° C.

It has been shown that carbon at 1500° C. gives the first form of carbon-spectrum, and that the same spectrum is given by the electric spark in either carbonic oxide or olefiant gas at the ordi-

* I have repeatedly observed this increase of resistance in the experiment on condensed gases. The spark which passed with ease in carbonic anhydride at the ordinary pressure could hardly be got through the gas at 7 atmospheres' pressure, while there was no perceptible increase in the resistance afforded by hydrogen when the pressure was increased to 9 atmospheres; and the spark which passed with ease through 7 millims. in hydrogen at 9 atmospheres' pressure would hardly pass through 5 millims. in cyanogen at the ordinary pressure, and through only a fraction of a millimetre in cyanogen at 4 atmospheres' pressure.

nary pressure, but that when the pressure of the gas is increased the temperature of the spark rises. When, then, we find that on gradually diminishing the pressure the same spectrum is given until the pressure falls to about 100 millims., and then suddenly changes to the third form, we can hardly resist the conclusion that this third form of carbon-spectrum is due to carbon rendered luminous below 1500° C. The result that the temperature of the discharge in a vacuum-tube may be below 1500° C. is certainly unexpected, but it can hardly be rejected* unless we give up the attempt to account for the differences in the spectra of the same element by differences in the temperature of ignition. We may, of course, suppose the existence of allotropic modifications of carbon-vapour, but we have no proof of the existence of such.

The explanation of the Bessemer-flame is extremely difficult. I have endeavoured to obtain some approximation to the temperature of the flame both by calculation and by experiment. The calculation is based upon the composition of the gas issuing from the convertor. A sample of the gas collected from the converter at the Steel-works at Crewe was analyzed by Mr. C. R. A. Wright, B.Sc., and gave the following result:—

Carbonic anhydride . . .	3·78
Carbonic oxide	16·20
Oxygen	0·57
Nitrogen	79·44
	99·99

The temperature is calculated on the assumption that the oxygen of the air is used up in burning the carbon of the cast iron to carbonic oxide and carbonic anhydride, and in burning the iron to ferroso-ferric oxide.

litres.			grs.		grs.	
3·78	carbonic anhydride	weigh	7·43	and contain	2·03	carbon.
16·20	carbonic oxide	„	20·27	„	8·69	„
0·57	oxygen	„	0·82			
79·44	nitrogen	„	99·92			

The total volume of oxygen contained in the gaseous products of combustion is

* Wüllner (Pogg. *Ann.* Dec. 1868) regards the temperature in a hydrogen vacuum-tube as at a maximum when the tension is about 30 millims., being lessened either by increase or diminution of the pressure.

litres.	litres.
3·78 in	3·78 carbonic anhydride.
8·10 in	16·20 carbonic oxide.
0·57	
12·45	

But 79·44 litres of nitrogen are mixed in air with 21 litres of oxygen. Hence $21-12·45=8·55$ litres of oxygen have combined with iron.

The heat produced by the combustion is as follows:—

grs.			grs.	Thermal units.
2·03 carbon	burning to	CO^2 evolve	$2·03\times 8080=$	16402
8·69	„	CO „	$8·69\times 2474=$	21499
32·08 iron	„	Fe^3O^4 „	$32·08\times 1582=$	50778
				88679

The products of combustion and their specific heats are as follows:—

grs.		
7·43 CO^2	$\times 0·216$ =	1·60
20·27 CO	$\times 0·248$ =	5·03
44·26 Fe^3O^4	$\times 0·152$* =	6·73
99·92 N	$\times 0·244$ =	24·38
0·82 O	$\times 0·218$ =	0·18
		37·92

and the temperature of the flame is therefore

$$\frac{88679}{37·92}=2339° \text{ C.}†$$

The result of this calculation is, of course, open to the same objection as all calculated flame-temperatures, that no allowance can be made for dissociation. It is too high also for another reason—that a very considerable part of the heat produced is expended in heating up the molten metal itself, which is immensely hotter at the end of the blow than it is at the beginning. If we assume that, together with the quantities given above, we have 300 grs. iron heated up from 1000° C. to the temperature

* $\dfrac{3\times 6·4+4\times 4}{\text{at. wt. } Fe^3O^4}=0·152.$

† This calculation represents 10 grs. carbon burnt for 32 grs. iron. Assuming the pig-iron to contain 3 per cent. carbon, this would give a loss of 32 iron for $\frac{100}{3}\times 10=333$ pig iron, or about 10 per cent. The average loss from all causes is reckoned, I believe, at about 15 per cent.

of the flame (which is, of course, not really the case), we obtain as the temperature of the flame 1700° C. instead of 2339° C.

Mr. Ramsbottom has kindly placed at my disposal the result of an experiment made at Crewe to determine the "heat of the Bessemer-flame, in which it was found that on exposing a bar of cast iron (quality not stated), $1\frac{1}{8}$ inch in diameter, to the action of the flame at a distance of about 12 inches from the mouth of the vessel, it began to melt in about $5\frac{1}{2}$ minutes, the iron dropping off in small globules at the rate of about 30 or 40 per minute. A bar of wrought iron exposed in a similar manner for about six minutes at the end of the blow did not melt."

We may therefore conclude that the temperature of the Bessemer-flame lies between 1000° C. and 1500° C. It is worthy of remark (since it throws light on the question whether the carbon-spectra are to be regarded as produced by carbon in the gaseous state or not) that the Bessemer-spectrum contains the lines of iron. There is probably as much difficulty in supposing the existence of iron-vapour below 1500° C. as in supposing the existence of carbon-vapour at the same temperature.

The Bessemer-spectrum is either due to carbon or to carbonic oxide. If it be produced by carbon, we are compelled to admit the existence of two spectra produced by carbon at the same temperature; for the Bessemer-flame lies between 1000° C. and 1500° C., and the gas of the vacuum-tube is below 1500° C. If we assume that the Bessemer-spectrum is due to carbonic oxide, we have to explain why in the Bessemer-flame carbonic oxide gives a spectrum consisting of bright lines, and in the carbonic-oxide flame a continuous spectrum. Deville* has shown that the carbonic-oxide flame varies in temperature from about 1000° C. at the top of the flame to a temperature considerably above the fusing-point of platinum, or probably 2500° C. at the blue cone 10 millims. from the jet; so that we have then to admit the existence of two spectra of carbonic oxide within the same range of temperature. It may be objected that the determination of temperature is very uncertain, and that if carbonic oxide were more intensely heated it would give out the Bessemer-spectrum; and indeed at the highest temperature obtainable from carbonic oxide and oxygen burnt together at the oxyhydrogen jet a faint spectrum does become visible from the blue cone, which Deville has shown to possess the highest temperature; but it is identical with the carbon-spectrum No. 1. The probability is that the compound carbonic oxide, like the compound carbonic anhydride, always gives a continuous spectrum—but that at the extremely high temperature obtained in the experiment mentioned above the carbonic oxide

* *Leçons sur la Dissociatiom,* p. 302.

becomes dissociated, and the carbon set free then gives off the ordinary carbon-spectrum.

In conclusion, my best thanks are due to Professor Roscoe for valuable advice and assistance rendered me in this investigation.

XXXI. *On the Cause of the Phenomena of Voltaic Cooling and Heating discovered by* Peltier. *By* E. Edlund*.

If a voltaic current passes through a metal conductor, heat is developed, and its quantity is proportional to the resistance and the square of the intensity. An exception to this rule, however, is formed by the place of junction of two heterogeneous metals. Peltier showed, so long ago as 1834†, that the solderings between two different metals become either colder or warmer than the other parts of the conductor, according to the direction in which the current traverses the places of contact. Peltier found that the strongest action was that between bismuth and antimony. If the current passed through the junction from the bismuth to the antimony there was a fall of temperature, while in the opposite case there was an increase. These experiments were confirmed by Moser ‡. Lenz subsequently§ gave this experiment an attractive form by showing that at the place of contact between bismuth and antimony water can be made to freeze if a feeble current passes from the former to the latter metal and both have been previously cooled in a mixture of ice and water.

Peltier was led by his experiments to the view that these phenomena of cooling and heating are closely connected with the electrical conductivity of the metals. When the current passes from a worse to a better conductor, in his opinion the temperature at the soldering is higher than when the current goes in the opposite direction. E. Becquerel, however, has shown‖ that this is not always the case, and that therefore the voltaic resistance is of no importance from this point of view. He made special experiments to ascertain whether at the point of contact the voltaic resistance was in any manner dependent on the direction of the current, so that in one case it should be greater and in another smaller than in the other parts of the circuit. But the experiments gave a negative result; the observed differences in the resistance, according to the direction of

* Translated from Poggendorff's *Annalen*, having been read before the Swedish Academy of Sciences at Stockholm, April 14, 1869.

† *Ann. de Chim. et de Phys.* vol. lvi. p. 371.

‡ *Repertorium der Physik*, vol. l. p. 349.

§ Pogg. *Ann.* vol. xliv. p. 342.

‖ *Ann. de Chim. et de Phys.* S. 3. vol. xx. p. 53.

the current, were not more than might be assumed to arise from the differences in temperature at the points of contact. The experiments thus did not at all prove that the cooling and heating observed by Peltier had anything to do with the voltaic conducting-power. It is also clear that if the voltaic resistance were indeed different with the direction in which the current traversed the point of contact, it would certainly follow that the degree of heating might vary with the direction of the current, but in no case could there be a cooling or real absorption of heat. Becquerel was of opinion, however, that these experiments indicated another connexion between the phenomena in question and previously well-known voltaic phenomena; for he found that when the voltaic current which traverses the place of contact has the same direction as the thermoelectric current which would be formed by heating the junction, the temperature diminishes at the place of contact, but that when the current is in the opposite direction there is a rise in temperature. The phenomena in question would thus have a connexion with the thermoelectric properties of bodies. How far this conclusion is right or not under all circumstances can only be definitely settled when a larger number of metals and alloys have been investigated with this view.

G. v. Quintus Icilius has made careful examination of the quantitative relations of these phenomena, from which it resulted that the difference in temperature produced by the current at the junctions of a thermoelectric pile of bismuth and antimony was proportional to the intensity of the current. Hence these phenomena follow a totally different law from that of the ordinary thermal action of the voltaic current; for while the former are simply proportional to the intensity, the latter thermal action is proportional to the square of the intensity. The accuracy of this result has been confirmed by Frankenheim's investigation*, which was made in a manner totally different from the above. Hence it may be regarded as demonstrated that the variations in temperature in question are proportional to the intensity of the current by which they are caused.

It is in itself a very remarkable fact that under certain circumstances the voltaic current can produce an absorption of heat; for its ordinary action is to produce heat. Hence I have thought that an account of the cause of this deportment might have some interest; for, as will afterwards be shown, Peltier's phenomena of cooling and heating may be easily deduced from the idea of electromotive force; their existence may be proved to be absolutely necessary; so that they might have been discovered *à priori* if their existence had not previously been demon-

* Pogg. *Ann.* vol. xci. p. 161.

strated. The proof rests upon the general principles which have been introduced into science by the mechanical theory of heat.

An electromotive force, like any other natural force, cannot produce mechanical work out of nothing. The well-known principle, *ex nihilo nihil fit*, finds everywhere a confirmation. Electromotive forces are only "transforming forces," which change one kind of motion into another, and always in such a manner that the kind of motion which is changed has the same mechanical value as that into which it is changed; they are mechanically equivalent to each other. If a closed conducting-wire is brought near a voltaic current or is removed from it, induction-currents are formed in the conducting-wire; and a certain amount of work is required to effect this approximation or removal. By the force of induction this work is changed into electricity, which in turn produces a quantity of heat, which, as I have elsewhere* shown, constitutes the mechanical equivalent of the work used. If one soldering of a closed ring consisting of two metals be heated, a thermoelectric current is formed which produces heat in the conductors which it traverses. But this heat cannot be produced from nothing. The mechanical theory of heat requires that just as much heat shall disappear at the heated junction, or, to speak more correctly, be changed into electricity. When the temperature has become the same at both junctions and the thermoelectric current has ceased to circulate, as much heat will have been developed in the circuit as has been changed into electricity at the point of contact. Hence work has neither been produced nor destroyed by the thermoelectric current. If we join by a metallic wire the poles of an electromotor, for instance a voltaic battery, in which chemical combinations result from the action of the current, an amount of heat is produced which is proportional to the square of the intensity, and to the entire resistance in the battery and in the interpolar. Now, from the mechanical theory of heat, as much heat must disappear in the electromotor or be changed into electricity. If the heat resulting from the chemical combinations be designated by a, that produced in the electromotor by the action of the current by b, and that produced in like manner in the interpolar conductor by c, the quantity of heat produced in the electromotor will be equal to $(a+b)-(b+c)=a-c$. Hence the entire quantity of heat obtained in the electromotor and the interpolar conductor will be equal to that which would have been formed from the same chemical action without any current having been formed. The current, therefore, has neither produced nor consumed heat; the heat necessary for the production of the current was just as

* Pogg. *Ann.* vol. cxxiii. p. 193.

great as that which it produced by its passage through the circuit. Hence it has only transferred the heat from the electromotor to the interpolar conductor without any loss or gain of heat. That this conclusion is quite correct has been experimentally proved by Favre*. This distinguished physicist has proved that the amount of heat liberated by a voltaic element whose poles are connected by a conducting-wire of greater or less resistance agrees quite accurately with the amount of heat which the operations which have taken place in the battery would have furnished if no current had been formed. The heat obtained in the interpolar conductor, together with that which appears in the battery itself, form a total amount of heat which is equal to that produced by the chemical action. The current has neither increased nor diminished this quantity of heat. Hence, as was remarked above, in a thermoelectric pile which is unaccompanied by any chemical action, the total amount of heat produced must be null. I will now apply these principles to the phenomena of cooling and heating discovered by Peltier.

2. Assuming we have an electromotor of any quality, the poles of which are connected with each other by a conductor, if the electromotive force is e, and the entire resistance in the electromotor together with that in the conductor is equal to l, the total quantity heat evolved by the current is $\frac{e^2}{l^2}l = e\frac{e}{l}$, or, if s is the intensity, $=es$. But, from what has been said, as much heat must disappear in the electromotor or be converted into electricity. Hence there must be an absorption of heat which is proportional to the electromotive force multiplied by the intensity of the current. If there are two electromotors whose electromotive forces are $e+e'$, and these both act in the same direction, the entire quantity of heat developed by the current is $\frac{(e+e')^2 l_1}{l_1^2} = (e+e')s_1$, if s_1 and l_1 denote respectively the intensity and the resistance. Hence this quantity of heat must be absorbed in the two electromotors together. It follows thence that in each electromotor there must be an absorption of heat which is proportional to the common intensity multiplied by the electromotive force. The result will, of course, be the same if there is a larger number of electromotors, provided only they act in the same direction.

If the electromotive forces act in opposite directions and e is greater than e_1, a current is obtained in the direction of the first force; the total quantity of heat developed by the current is $=(e-e_1)s_{\prime\prime}$ when the intensity is $s_{\prime\prime}$; and just this quantity of heat must disappear in the two electromotors. But in the first the

* *Ann. de Chim. et de Phys.* S. 3. vol. xl. p. 293.

quantity of heat es_{II} will be absorbed, which is greater than that produced by the current. The difference between the two, or $e's_{II}$, must therefore be *produced* in the other electromotor, so that the algebraic sum of that which is produced and of that which disappears may be equal to zero. It therefore follows that if a current traverses an electromotor in the opposite direction to the current which is produced by it, heat is developed in this electromotor proportional to the product of the electromotive force into the intensity. Hence is obtained the final result:—*If a voltaic current traverses an electromotor in the same direction as the current which is produced by the electromotor, absorption of heat ensues; if the current is in the opposite direction, heat is produced; the quantity of heat which is absorbed in the first case and produced in the latter is proportional to the intensity of the current multiplied by the electromotive force at the place where the change of heat ensues.*

If two heterogeneous metals are brought into contact with one another, an electromotive force ensues at the point of contact. If a voltaic current traverses the place of contact, there must either be absorption or production of heat. Here, then, we have the cause of Peltier's phenomena. The quantities of heat absorbed in the one and produced in the other case are proportional to the product of the intensity into the electromotive force. Hence, if with different intensities experiments are made with the same two metals, the differences in temperature must be proportional to the intensities, as has already been experimentally shown. But if, retaining the same intensity of the current, experiments are made with different metals, the quantities of heat must be proportional to the electromotive forces. Hence by measuring the quantities of heat we should be in a posit.on to arrange the metals in the actual electromotive series. But this series must be quite different from that obtained when the metals are arranged according to the observed differences in temperature; for these differences, besides depending on the quantities of heat absorbed and produced, depend also on the thermal capacities of the metals, on the greater or less degree of cooling during the experiment, and so forth. All experimenters who have worked at this subject have found the difference in temperature to be greatest at the contact of bismuth and antimony; but this by no means proves that the contact between these metals produces the greatest electromotive force. The difference in temperature must, as has been said, depend essentially on the capacity for heat. Comparing the thermal capacities for equal volumes of the metals with which Peltier's experiments were made, it is found that bismuth has the least capacity of all metals, and next to it antimony. Hence, when the current passes,

the contact between these two metals must show relatively great variations in temperature, without these indicating any considerable electromotive force between them.

If the metals are arranged according to the quantities of heat which are absorbed or produced in case a voltaic current traverse the place of contact, it does not seem to me that it is *à priori* certain that we should obtain the same series as that formed when they are arranged according to their electrical tension on contact. It seems conceivable that the magnitude of the current which a contact can produce does not depend simply on the tension which the electricity can attain when the insulated metals are placed in contact, but also on the time necessary for the production of this state. Though this time is certainly very short, it may doubtless be comparable with the time for the passage of the current from one pole to the other. If it is indeed so, the ordinary electrical series for the case in which a real current is produced cannot without further proof be regarded as the right one. What is the real state of the case must be decided by trustworthy measurements of the heat absorbed and produced. Peltier's phenomena obtain thus an unexpected interest. If time and circumstances permit, I hope before long to make an experimental determination of the quantities of heat in question.

XXXII. *Comparison of a Theory of the Dispersion of Light on the Hypothesis of Undulations with* Ditscheiner's *determinations of Wave-lengths and corresponding refractive Indices.* *By* Professor CHALLIS, *M.A., F.R.S., F.R.A.S.**

THE Theory of the Dispersion of Light which I proposed in this Journal in 1864 is, I believe, the only one which may be strictly said to rest on the hypothesis of undulations. It was commenced in the Number for June of that year; and in the Supplementary Number for December it is compared with the refractive indices of two substances for seven principal rays, Fraunhofer's values of the wave-lengths of the rays being adopted. At the end of an article on the Undulatory Theory of Light in the Philosophical Magazine for May 1865 the same comparison is made by means of Ångström's values of λ for the same rays. The theory is reproduced in my work 'On the Principles of Mathematics and Physics'—at first, just as it was originally proposed; but subsequently, while the work was in the press, it occurred to me that a course of reasoning somewhat different in principle would be more exact, and, accordingly, by another in-

* Communicated by the Author.

vestigation (in pages 421–426) I obtained a new formula for dispersion. The numerical results from the two investigations (exhibited in page 427) show that the second formula accords with the experiments in a slight degree better than the first.

Since the publication of that volume I have become acquainted with Ditscheiner's measures of a large number of wave-lengths for dark rays of the solar spectrum, and of corresponding refractive indices; and my present object is to compare these data with the theory modified as above stated. Ditscheiner's measures, accompanied by investigations of appropriate formulæ, are given in a memoir in the *Sitzungsberichte* of the Mathematico-physical Class of the Imperial Academy of Sciences at Vienna (vol. l. part 2 (1864), p. 296). The values of λ were determined, according to Fraunhofer's method, by the diffraction-spectrum. The mean interval between the lines of the grating, in default of means of measuring it directly, was, at first, inferred, by observation and calculation combined, from Fraunhofer's determination of the value of λ for that component of the double line D which is nearest the violet end of the spectrum; and the wave-lengths obtained for the other lines were thus made dependent on that determination. Subsequently, having learnt that Ångström had employed a value of the interval between the lines of his grating obtained by direct mechanical means, Ditscheiner succeeded in effecting a like determination with respect to his own grating, and was thus enabled to calculate independent values of all the wave-lengths. The results of this calculation, which differ but little from those previously obtained, are given in the above-cited publication (vol. lii. part 2 (1865), p. 289). Those of these values to which there are corresponding determinations of refractive indices, the number of which is seventy-three, are used in the subjoined comparison with theory.

Before entering upon this comparison, I propose to give some account of the principles of the theory, and of the above-mentioned modification of it. The diminished rate of propagation of waves in transparent substances is ascribed to the obstacle to the free motions of the particles of the æther caused by reflections due to the incidence of the waves upon the atoms. These reflections are supposed to take place as if the fluid were incompressible; and as they would thus be transmitted instantaneously, the mean effect, at a given position, of the simultaneous reflections from a vast number of atoms may be conceived to bear a finite ratio to the incident velocity, even though the space occupied by atoms should be extremely small compared to the intervening space. It is presumed that that ratio may be the same at different parts of the same wave, and, consequently, that the retarding force due to the atoms has a constant ratio to the

effective accelerative force of the æther. Hence putting κ'^2a^2f for the latter force, $K\kappa'^2a^2f$ for the retarding force, and κ^2a^2f for the accelerative force of the æther due to the actual variations of density, we have

$$\kappa'^2a^2f=\kappa^2a^2f-K\kappa'^2a^2f, \text{ or } \kappa'^2(1+K)=\kappa^2.$$

Putting, therefore, μ for the ratio of κ to κ', which is the ratio of the rate of propagation outside the medium to the rate within, it follows that $1+K=\mu^2$. Hence, since the retardation must vary *cæteris paribus* as the number of atoms in a given space (that is, as the density of the medium), if we put δ for the density, and $H\delta$ for K, we get $\mu^2=1+H\delta$. The constant K, being by hypothesis the same for different parts of the same wave, will also be the same for waves of different breadths.

In this reasoning the atoms are regarded as fixed. Supposing, as must be the case, that they are moveable about their mean positions of equilibrium, the retardation due to the reflections from each atom will be altered in the ratio of the velocity of the æther relative to the atom to the actual velocity of the æther. That is, x being the distance at the time t of the centre of the atom from a fixed plane perpendicular to the direction of the propagation of the waves, and V the velocity of the æthereal particles at that distance, we shall have

$$\mu^2-1=H\delta\left(1-\frac{dx}{Vdt}\right),$$

the medium being supposed at present to be a simple one. In order, therefore, to obtain a formula for μ it is necessary to calculate $\frac{dx}{dt}$.

Now the velocity $\frac{dx}{dt}$ of the vibrating atom may be considered to result from three different actions:—(1) the distribution about the surface of the atom of the condensation and pressure due to the incidence of a given series of æthereal waves, which, in fact, is the primary cause of its movement; (2) the resistance of the æther to the motion of the atom; (3) the action of the proper molecular forces of the medium called into play by the displacement of the atom. In my original researches I supposed that the first of these actions depended on the relative motion of the atom and the æther; but afterwards it occurred to me to reason as follows. The atom being supposed to have a vibratory motion from any cause, conceive to be impressed upon it and upon the *whole* of the fluid at each instant this motion in the opposite direction. The atom will thus be brought to rest; and as the motion and propagation of the waves will in no manner be

affected by a motion common to all the parts of the fluid, they will be incident on the atom just as before, excepting that by reason of this common motion a given condensation will reach a given point of space a little earlier or a little later than it otherwise would. As the effect of this inequality, as far as regards the action on the atom, is a quantity of the second order, it may be neglected in this investigation. Consequently the distribution of condensation about the surface of the atom is to be determined just as if the atom were fixed.

The problem for the case of the fixed atom is discussed in the Number of the Philosophical Magazine for May 1866 (pp. 353–360), and in 'The Principles of Mathematics' (pp. 279–287 & 441–446); and the expression obtained for the accelerative action on the atom, insignificant terms being omitted, is

$$\frac{3\mathrm{H}_1}{2\Delta}\cdot\frac{d\mathrm{V}}{dt},$$

Δ being the ratio of the density of the atom to that of the æther, and H_1 a certain constant factor depending in an unknown manner on the breadth of the undulations.

The resistance of the æther to the motion of the atom may be at once inferred from the solution of the well-known problem of the resistance of the air to the motion of a ball-pendulum; and accordingly the part of the accelerative action due to this cause is $-\frac{1}{2\Delta}\cdot\frac{d^2x}{dt^2}$.

The molecular force of the medium called into action by the *relative* displacement of its atoms will, when the condition of *transparency* is satisfied, have a fixed ratio to the actual acceleration of the atom. I have therefore given it the expression $\frac{e^2}{\kappa'^2a^2}\cdot\frac{d^2x}{dt^2}$, the constant e^2 depending on the proper molecular elasticity of the medium.

From these considerations it follows that

$$\frac{d^2x}{dt^2}=\frac{3\mathrm{H}_1}{2\Delta}\,\frac{d\mathrm{V}}{dt}-\frac{1}{2\Delta}\,\frac{d^2x}{dt^2}+\frac{e^2}{\kappa'^2a^2}\,\frac{d^2x}{dt^2}$$

Hence, supposing V and $\frac{dx}{dt}$ to vanish at the same time, which is another necessary condition of transparency, we have by integrating,

$$\frac{dx}{\mathrm{V}\,dt}=\frac{3\mathrm{H}_1\kappa'^2a^2}{(1+2\Delta)\kappa'^2a^2-2\Delta e^2}\,.$$

It appears from reasoning contained in the discussions above mentioned, that the constant H_1 is equal to unity for an incom-

pressible fluid, and that for a compressible fluid it is different for different values of λ. According to the adopted hydrodynamical principles, this quantity becomes a function of λ only in consequence of the effect produced on the distribution of condensation about the surface of the atom by lateral spreading due to transverse vibrations, these vibrations being brought into action by the disturbance of the waves caused by their incidence on the atom. I have not succeeded in obtaining by *à priori* investigation an exact expression for the condensation thus modified; but from the general expression for the condensation in transverse vibrations I have inferred that the distribution of condensation in this case must be a function of $\frac{1}{\lambda'^2}$, λ' being the effective breadth of the waves. (See Phil. Mag. (Supplement) for December 1864, p. 500, and 'Principles of Mathematics,' p. 370.)

Accordingly it has been assumed that, to a first approximation,

$$\mathrm{H}_1 = k\left(1 - \frac{k'}{\lambda'^2}\right),$$

k and k' being unknown physical constants. Consequently, since $\lambda = \mu\lambda'$ and $\kappa = \mu\kappa'$, the formula for dispersion in a *simple* medium becomes

$$\frac{\mu^2-1}{\mathrm{H}\delta} = 1 - \frac{dx}{\mathrm{V}\,dt} = 1 - \frac{3\kappa^2 a^2 k\left(1 - \frac{k'\mu^2}{\lambda^2}\right)}{\kappa^2 a^2(1+2\Delta) - 2\Delta e^2\mu^2}.$$

The same form of expression applies to a *compound* medium, as is shown in 'The Principles of Physics,' pp. 429 & 430. In the existing state of physics it does not appear possible to obtain, either by theoretical calculation or by experiment, the values of the constants H, k, k', Δ, and e^2. But since the equation may be put under the form

$$\mu^2 + \frac{\mathrm{A}'}{\mu^2} + \frac{\mathrm{B}'}{\lambda^2} = \mathrm{C}',$$

the values of A′, B′, and C′ may be found by means of three sets of corresponding values of μ and λ given by observation. The formula may then be employed to calculate values of λ from other given values of μ; and a comparison of the results with the corresponding observed values of λ will, in proportion to the degree of accordance, be evidence of the truth of the theory.

Having gone through such calculations by making use of the before-mentioned values of μ and λ obtained by Ditscheiner, I have collected the results in the annexed Table, in which also Kirchhoff's measures are inserted for the sake of identification of the lines. Instead of calculating the constants A′, B′, C

from the values of μ and λ for Fraunhofer's lines B, E, H, which would probably be the most favourable for obtaining results in accordance with observation, in order to put the theory to a severer test I have calculated, first, with the data for the lines B, E, G, and then with those for C, F, H. As the comparisons in the two cases would necessarily be affected by errors in the data, and as I had no reason to prefer one set of data to the other, I have considered the mean between the values of λ given by the two calculations to be a more correct expression of the theoretical result than either value taken separately.

By the preliminary calculations the constants A$'$, B$'$, C$'$ were determined as follows :—

By 1, $\log A'=1{\cdot}0703283$, $\log B'=0{\cdot}3013700$, $C'=7{\cdot}161816$.

By 2, $\log A'=1{\cdot}0604669$, $\log B'=0{\cdot}2746509$, $C'=7{\cdot}057775$.

Designation of ray.	Kirchhoff's measure.	Ditscheiner's refractive index.	Ditscheiner's wave-length.	Excess of calculated wave-length.		
				By first calculation.	By second calculation.	Mean.
B	593·0	1·61358	68833	0	+117	+ 58
C	694·0	1·61537	65711	− 80	0	− 40
	877·0	1·61824	61470	−106	− 66	− 86
D	1004·8	1·62020	59021	−100	− 78	− 89
	1135·0	1·62166	57193	+ 92	+104	+ 98
	1207·5	1·62274	56240	− 70	− 64	− 67
	1280·0	1·62363	55368	− 64	− 64	− 64
	1324·8	1·62415	54854	− 37	− 39	− 38
	1351·3	1·62448	54549	− 34	− 37	− 35
	1389·6	1·62494	54132	− 28	− 33	− 31
	1421·6	1·62530	53792	− 3	− 9	− 6
E	1523·5	1·62650	52783	0	− 11	− 5
	1577·5	1·62705	52349	− 7	− 19	− 13
	1634·0	1·62760	51912	+ 2	− 12	− 5
	1648·8	1·62775	51809	− 10	− 24	− 17
	1655·6	1·62782	51754	− 8	− 22	− 15
	1693·8	1·62817	51503	− 21	− 36	− 28
	1750·4	1·62872	51068	+ 9	− 8	+ 1
	1777·4	1·62897	50879	+ 17	0	+ 8
	1834·0	1·62953	50493	+ 5	− 13	− 4
	1885·8	1·63003	50145	+ 8	− 11	− 2
	1920·0	1·63038	49914	+ 1	− 18	− 8
	1961·0	1·63075	49653	+ 15	− 5	+ 5
	1989·5	1·63113	49412	+ 7	− 13	− 3
	2005·0	1 63133	49269	+ 21	0	+ 10
	2041·4	1·63177	48990	+ 19	− 2	+ 8
	2067·0	1·63205	48791	+ 42	+ 20	+ 31
F	2080·1	1·63225	48687	+ 22	0	+ 11
	2119·8	1·63269	48317	+122	+100	+111
	2187·1	1·63390	47717	+ 7	− 17	− 5

Table (*continued*).

Designation of ray.	Kirchhoff's measure.	Ditscheiner's refractive index.	Ditscheiner's wave-length.	Excess of calculated wave-length. By first calculation.	By second calculation.	Mean.
	2233·7	1·63446	47371	+ 33	+ 9	+ 21
	2264·3	1·63492	47106	+ 42	+ 17	+ 30
	2309·0	1·63560	46742	+ 35	+ 10	+ 22
	2416·0	1·63718	46097	−143	−169	−156
	2436·5	1·63743	45901	− 73	− 99	− 86
	2467·4	1·63789	45606	− 7	− 32	− 20
	2489·4	1·63818	45409	+ 49	+ 23	+ 36
	2537·1	1·63886	45089	+ 42	+ 17	+ 30
	2566·3	1·63928	44880	+ 54	+ 28	+ 41
	2606·0	1·63986	44633	+ 33	+ 7	+ 20
	2627·0	1·64013	44498	+ 45	+ 20	+ 32
	2638·6	1·64031	44418	+ 44	+ 18	+ 31
	2670·0	1·64080	44222	+ 22	− 4	+ 9
	2686·6	1·64101	44121	+ 30	+ 5	+ 17
	2721·6	1·64150	43908	+ 30	+ 5	+ 18
	2734·9	1·64168	43813	+ 48	+ 23	+ 35
	2775·6	1·64224	43600	+ 23	− 2	+ 11
	2797·0	1·64251	43466	+ 44	+ 20	+ 32
	2822·8	1·64287	43314	+ 48	+ 23	+ 36
G	2851·7	1·64334	43170	0	− 24	− 12
	2869·7	1·64352	43070	+ 27	+ 3	+ 15
a	....	1·64369	42953	+ 76	+ 52	+ 64
A	....	1·64421	42789	+ 34	+ 10	+ 22
γ	....	1·64448	42668	+ 50	+ 26	+ 38
δ	....	1·64476	42555	+ 54	+ 31	+ 42
ϵ	....	1·64511	42425	+ 50	+ 27	+ 38
B	....	1·64536	42325	+ 55	+ 32	+ 43
ζ	....	1·64569	42238	+ 18	− 5	+ 7
θ	....	1·64606	42069	+ 49	+ 27	+ 38
ι	....	1·64630	41871	+159	+136	+148
κ	....	1·64687	41792	+ 31	+ 9	+ 20
μ	....	1·64742	41626	+ 1	− 22	− 11
ν	....	1·64771	41498	+ 27	+ 5	+ 16
Γ	....	1·64819	41392	− 35	− 56	− 45
E	....	1·64893	41077	+ 27	+ 7	+ 17
o	....	1·64941	40876	+ 67	+ 47	+ 57
π	....	1·64966	40829	+ 31	+ 11	+ 21
σ	....	1·65009	40685	+ 34	+ 14	+ 24
Z	....	1·65064	40506	+ 35	+ 16	+ 25
τ	....	1·65113	40392	− 7	− 26	− 17
ϕ	....	1·65194	40117	+ 15	− 3	+ 6
H	....	1·65317	39742	+ 17	0	+ 8
H′	....	1·65435	39405	+ 8	− 8	0

In the foregoing Table Fraunhofer's principal rays are indicated, as usual, by the letters B, C, D, E, F, G, H. The other letters were employed by Ditscheiner to designate lines for which there were no measures by Kirchhoff. The numbers opposite to

the double line D apply to the mean position between the components. The refractive index corresponding to the measure 1989·5 has been altered conjecturally, the given value (1·63133) having been the same as that corresponding to the measure 2005·0.

Respecting the numbers in the last four columns, it is to be stated that they express *actual lengths in millimetres multiplied by* 10^8. It will hence be seen that the differences between the calculated and observed values of λ are generally very small. The larger differences occur so exceptionally that they must plainly be referred to errors of the data from observation. This is especially the case with respect to the rays whose measures by Kirchhoff are 1135·0, 2119·8, 2416·0, and 2436·5, and the ray designated by the letter ι. Leaving out of account the discordant results for the ray 1135·0, there seems to be a systematic variation between the calculated and observed wave-lengths in the part of the spectrum from B to E, but not nearly in the same degree in any other part. Also it is to be noticed that there is a close agreement between the results from the two calculations, the difference in no case exceeding 26, excepting in the first three comparisons, for which the differences are respectively 117, 80, and 40. This circumstance might be supposed to indicate a discrepancy in the data for the rays B and C.

In order to clear up this point, I went through for the seven principal rays the same calculations as those by which the Table above was constructed, only using, instead of Ditscheiner's values of λ, those given by Ångström in his *Recherches sur le Spectre solaire,* pp. 31 & 32. The results in the two preliminary calculations of the constants A′, B′, C′ were

$$\log A' = 1{\cdot}0870469, \quad \log B' = 0{\cdot}3399332, \quad C' = 7{\cdot}343192;$$
$$\log A' = 1{\cdot}0576341, \quad \log B' = 0{\cdot}2657302, \quad C' = 7{\cdot}028368.$$

The excesses of the calculated values of λ resulted as follows:—

Ray.	Ångström's wave-length.	Excess of calculated wave-length.			Former mean.
		By first calculation.	By second calculation.	Mean.	
B	68671	0	+191	+95	+58
C	65621	−131	0	−65	−40
D	58921	−108	− 67	−87	−89
E	52691	0	− 4	− 2	− 5
F	48607	+ 13	0	+ 6	+11
G	43073	0	+ 3	+ 2	−12
H	39681	− 32	0	−16	+ 8

Hence it appears that the excesses for the rays B, C, D follow

nearly the same law as in the former comparison, and that the differences between the results of the first and second calculations are, for these three rays, even greater than before. These inferences make it probable that the discrepancies are not due to error in Ditscheiner's wave-lengths for the rays B and C.

I next performed the same calculations with Fraunhofer's values of μ for flint-glass No. 13 and Ditscheiner's values of λ, and obtained the following results:—

By first calculation,

$$\log A' = 1{\cdot}1982448, \quad \log B' = 0{\cdot}5816970, \quad C' = 8{\cdot}687700;$$

by second calculation,

$$\log A' = 1{\cdot}1255825, \quad \log B' = 0{\cdot}4350178, \quad C' = 7{\cdot}746712.$$

Ray.	Value of μ.	Value of λ.	Excess of calculated wave-length.		
			By first calculation.	By second calculation.	Mean.
B	1·62775	68833	0	+ 32	+ 16
C	1·62968	65711	+ 7	0	+ 4
D	1·63504	{ 59053	−88	−147	−117 }
		{ 58989	−24	− 83	− 53 }
E	1·64202	52783	0	− 65	− 32
F	1·64826	48687	+47	0	+ 23
G	1·66029	43170	0	+ 10	+ 5
H	1·67106	39742	−69	0	− 35

In this case there is not the same discrepancy between the comparisons for the rays B and C as in the two former calculations, and the law of the mean excesses is in some degree altered. It must not, however, be concluded that the previous discordances arose from inaccuracy in either or both of Ditscheiner's values of μ for those rays, because it is possible that differences in the character of the results may be due to differences in the qualities of the glasses employed, and that the dispersion-formula, which can only be regarded as approximate, may apply more accurately in proportion as the refractive and dispersive powers are larger. This point will be adverted to again presently.

It being uncertain to which of the two lines D Fraunhofer's determination of μ applies, I have compared the calculated value of λ with the observed value for each line. The excesses, given above within brackets, show that the more refrangible line is considerably more in accordance with the theory than the other.

The calculations were then repeated with the same values of μ and with Ångström's values of λ already cited, and the wavelength obtained for D was compared, as above, with the observed

wave-lengths of both lines, viz. 58951 and 58891, the mean between which was used in the previous comparison. The results from the two sets of data were as follows:—

$$\log A' = 1\cdot2351358, \quad \log B' = 0\cdot6161311, \quad C' = 9\cdot229205;$$
$$\log A' = 1\cdot1215922, \quad \log B' = 0\cdot4245740, \quad C' = 7\cdot699399.$$

Ray.	Excess of calculated wave-length.		
	By first calculation.	By second calculation.	Mean.
B	0	+106	+ 53
C	−43	0	− 21
D	−94	−135	−114
	−34	− 75	− 54
E	0	− 58	− 29
F	+37	0	+ 18
G	0	+ 38	+ 19
H	−118	0	− 59

Here again the mean excesses for B and C are more accordant than those deduced by the former calculation from Ditscheiner's values of μ and the same values of λ. Also the law of the mean excesses agrees generally with that of the means obtained by the next preceding calculation, although their amounts are somewhat larger. As the more refrangible of the lines D again gives more consistent results than the other, the theory, I think, may be considered to have decided that this line was bisected by Fraunhofer. In future calculations I shall assume that this was the case.

It remains to discuss more particularly the consequences of applying the dispersion-formula to substances of different densities and different refractive powers. With this object in view I begin with comparing Ditscheiner's values of λ for the seven principal rays (that for D being 58989), with values calculated by the formula from Fraunhofer's refractive indices for flint-glass No. 23 (prism of 60°) and flint-glass No. 3. The specific gravities of the two substances are respectively 3·724 and 3·512 (that of No. 13 is 3·723). In these two instances the calculation of A′, B′, C′ was made from one set of data, viz. the observed values of μ and λ for the rays B, E, G. The following results were obtained, $C_\lambda - O_\lambda$ signifying the excess of the calculated above the observed value of λ:—

For No. 23,

$$\log A' = 1\cdot0667953, \quad \log B' = 0\cdot2920263, \quad C' = 7\cdot095094;$$

for No. 3,

$$\log A' = 1\cdot0581414, \quad \log B' = 0\cdot2846254, \quad C' = 7\cdot061636.$$

Ray.	Flint-glass No. 23. Value of μ.	$C_\lambda - O_\lambda$.	Flint-glass No. 3. Value of μ.	$C_\lambda - O_\lambda$.
B	1·62660	0	1·60204	0
C	1·62847	6	1·60380	−128
D	1·63367	−36	1·60849	−131
E	1·64050	0	1·61453	0
F	1·64676	+ 6	1·62004	+ 48
G	1·65885	0	1·63077	0
H	1·66969	+23	1·64037	− 28

Here it is observable that the values of $C_\lambda - O_\lambda$ for No. 23, like those for the similar substance No. 13, are very small, and considerably less than the values for No. 3. The law of the excesses of calculation for the latter substance is nearly the same as that of the excesses deduced with the same values of λ from Ditscheiner's values of μ, but they are of larger amount, at the same time that the refractive indices are less. It seems, therefore, that the dispersion-formula becomes inexact in proportion as the refractive power of the substance is less than that of No. 13 or No. 23. I found, in fact, on applying it, just as in the last two instances, to Fraunhofer's crown-glass No. 13, the specific gravity of which is 2·535, and the refractive and dispersive powers very low, that it altogether failed. Yet, since the results of the other calculations seemed to indicate generally a *systematic* deviation of the calculated from the observed values of λ, there was a probability that the deviations were such as might be corrected by a more approximate formula, and that the failure in the instance of the crown-glass might be due to inadequate approximation, and not to error in the principles on which the formula was founded. In order to obtain a nearer approximation I reasoned as follows.

If the principles of the theoretical investigation be true, the variations of μ^2 for a given substance depend wholly on variations of $\frac{1}{\lambda^2}$; that is, μ^2 is a function of $\frac{1}{\lambda^2}$ and constants. We may therefore assume that

$$\mu^2 = A_0 + \frac{A_1}{\lambda^2} + \frac{A_2}{\lambda^4} + \frac{A_3}{\lambda^6} + \&c.$$

To ascertain the degree of approximation attainable by this series, I first applied it in the instance of the crown-glass No. 13, taking only the first three terms. The values of A_0, A_1, A_2, calculated from the subjoined values of μ and λ for the rays B, E, H, were found to be

$$A_0 = 2{\cdot}254474, \quad A_1 = [0{\cdot}4926929], \quad -A_2 = [1{\cdot}2120022].$$

Hence the following results were obtained, $C_\mu - O_\mu$ signifying the excess of the calculated above the observed value of μ:—

Ray.	μ.	λ.	$C_\mu - O_\mu$.
B	1·52431	6·8833	0·00000
C	1·52530	6·5711	−0·00288
D	1·52798	5·8989	−0·00142
E	1·53137	5·2783	0·00000
F	1·53434	4·8687	+0·00080
G	1·53991	4·3170	+0·00100
H	1·54468	3·9742	0·00000

The values of $C_\mu - O_\mu$ for the rays C, D, F, G, inasmuch as they correspond to large values of $C_\lambda - O_\lambda$, show that it is necessary to take into account a greater number of terms of the series. It was, in fact, to be expected, from what was said above, that an approximation could not be obtained by determining only three constants.

The above data for the rays B, D, F, H having been employed for calculating the constants of four terms of the series, the results were

$$A_0 = 2{\cdot}290385, \quad A_1 = [0{\cdot}2260364], \quad -A_2 = [0{\cdot}8234811], \quad A_3 = [1{\cdot}7942593].$$

Hence on calculating the values of μ for the rays C, E, G by means of these constants and the above values of λ for the same rays, the excesses $C_\mu - O_\mu$ to five places of decimals were found to be respectively +0·00001, 0·00000, −0·00006. These results prove that the relation between μ and λ for this substance is very closely expressed by taking account of only four terms of the series.

Lastly, I employed the same series to four terms to calculate $C_\mu - O_\mu$ for the rays C, E, G for water, the means (to five places of decimals) of two determinations of the refractive indices by Fraunhofer being adopted, viz.

$$B\mu = 1{\cdot}33096, \ C\mu = 1{\cdot}33171, \ D\mu = 1{\cdot}33358, \ E\mu = 1{\cdot}33585, \ F\mu = 1{\cdot}33780, \ G\mu = 1{\cdot}34128, \ H\mu = 1{\cdot}34417.$$

Calculations made with the data for B, D, F, and H gave

$$A_0 = 1{\cdot}748267, \quad A_1 = [0{\cdot}1049879], \quad -A_2 = [0{\cdot}9854382], \quad A_3 = [1{\cdot}8172302];$$

and the values of $C_\mu - O_\mu$ found for the rays C, E, G were respectively −0·00002, +0·00002, −0·00005. These differences,

which are of the same order as those between the different experimental determinations of μ, sufficiently attest the accuracy of the formula.

I take occasion to advert here to a memoir by the Astronomer Royal in the Philosophical Transactions for 1868 (part 1, p. 29), the object of which is to calculate the wave-lengths corresponding to Kirchhoff's scale-measures of lines of the spectrum, in order to increase the scientific value of these measures. The calculations for this purpose are based upon Ditscheiner's determinations of the wave-lengths for the lines B, C, D, E, F, G. Kirchhoff's measure is expressed as a function of the corresponding wave-length by a simple algebraical formula of interpolation containing six constants, the values of which are found by means of the scale-measures and wave-lengths of the above six lines. Mr. Airy chose this method because he did "not know any physical reason for adopting one formula in preference to another." The method appears not to have been successful, several of the differences between the computed and observed wave-lengths in the part of the spectrum between F and G ranging between 800 and 900, and in some cases exceeding the latter number. In the Table given in this communication, the greatest difference between the calculated and observed values of λ in the case in which the calculations were founded on the values of μ and λ for only the three lines B, E, G is 106, a few larger (evidently affected by errors of observation) being excepted. The superior accuracy of the results thus obtained is not to be attributed to my calculations having been made with refractive indices instead of Kirchhoff's measures, because these are data of the same kind as the others and equally trustworthy. My better success is rather to be accounted for by the advantage I have taken of the indications of the Undulatory Theory of Light, and may, I think, be justly regarded as some evidence of the truth of the proposed theory of Dispersion. Since Kirchhoff's scale-measure is a function of μ, the results of the foregoing calculations made by assuming for μ^2 a series proceeding according to powers of $\frac{1}{\lambda^2}$, would seem to prove that, by the intervention of a like series for the scale-measure, it would be possible to calculate the corresponding wave-length with great accuracy.

Cambridge, August 20, 1869.

XXXIII. *Observations of the Corona during the Total Eclipse, August 7th,* 1869. *By* Professor Edward C. Pickering*.

AMONG other expeditions to observe the recent eclipse was one under the direction of Professor Henry Morton, sent by the Nautical-Almanac Office to photograph the sun. I was attached to this party to make general and physical observations, and from our station at Mount Pleasant, Iowa, arrived at the following results.

It is commonly supposed that the light of the corona is polarized in planes passing through the sun's centre, and that it shines by reflected light. Wishing to verify this observation, I prepared an Arago's polariscope (in which the objects are viewed through a plate of quartz), and a double-image prism of Iceland spar. The two images appear of complementary colours when the light is polarized, the tint changing with the plane of polarization. I therefore expected to see two coloured coronas, the tint of each portion being complementary to that of the part at right angles to it, and the colour revolving with the polariscope. In reality the two images were pure white without any traces of colour; but the sky adjoining one was blue, adjoining the other yellow. As the instrument is of considerable delicacy, we must conclude that little or no polarized light is emitted by the corona. The sky adjoining it, however, is polarized in a plane independent of the position of the sun, since its colour (as seen in the polariscope) is the same whether above, below, or on one side of it. The most probable explanation of this curious phenomenon is, that the earth beyond the limits of the shadow, being strongly illuminated, acts as a new source of light, and thus gives rise to a polarization in a plane perpendicular to the horizon.

In hopes of determining the cause of discrepancy between this observation and those previously made, I have endeavoured to learn what form of polariscope has heretofore been used; but, unfortunately, in most cases no description has been published. One observer used a Savart's polariscope, and, holding it with its principal plane vertical, found strong traces of polarization in this plane. This observation, however, agrees with mine if we suppose that the polarization of the sky was taken for that of the corona, a natural mistake with this form of instrument. Another observer, who used a single plate of tourmaline, saw no evidence of polarization, that of the sky being too feeble to be perceived in this way. I verified my results with a simple prism of Iceland-spar, with which two images of the corona were seen precisely alike and showing no signs of polarization. We cannot infer from this that the corona is self-luminous, since polar-

* Communicated by the Author.

ization is produced only by specular and not by diffuse reflection.

The spectrum of the corona was observed in the following manner. A common chemical spectroscope was used; but instead of attaching it to a telescope, it was merely pointed in the proper direction a short time before totality. As its field of view was 7 or 8 degrees in diameter, the sun remained in it for a considerable time, and the spectrum obtained was that due to the corona, protuberances, and sky near the sun. On looking through the instrument during totality, a continuous spectrum was seen free from dark lines, but containing two or three bright ones—one near E, and a second near C. At the time, I supposed that these were due to the protuberances; but Professor Young, with a large spectroscope of five prisms, found a line near E which remained visible even when the image of the protuberance was moved off the slit, and therefore inferred that it was due to the corona. He also found the continuous spectrum free from dark lines—and that one, perhaps three of the bright lines coincide with those of the aurora borealis. These results would lead to the belief that the corona is self-luminous, the bright lines rendering its gaseous nature probable. If it is a part of the sun, even the remoter portions are one hundred times as near as the earth, and would receive ten thousand times as much heat, which would be sufficient to raise any known substance to incandescence.

Other observations, however, point to quite a different conclusion. A thermometer with blackened bulb was exposed to the sun's rays and the temperature recorded every five minutes. I found that it began to rise some time before contact, descending again as soon as the moon's limb became visible. It did not reach its former temperature until about a quarter of an hour after the eclipse began, or until a seventh of the sun's disk was obscured. The approach of the moon, therefore, appeared to cause an *increase* in the sun's heat. The amount of the change was only about 1°·3 C., the total difference between this thermometer and one in the shade being about 18° C., or in the ratio of 1 to 14. This fraction is but one-half of that given above, owing perhaps to the diminution of heat on the borders of the sun. During totality the difference between the two thermometers was almost nothing. In examining the photographs taken by the party, it was noticed that, while the light diminished near the edge of the sun, the moon's limb was very distinct, and that there was a marked increase in the light of the parts nearest it. It was suggested that this might be a subjective effect; but an examination of the photographs is sufficient to convince any one that the appearance is a real one. The glass

positives especially show that this effect extends over a large part of the sun's disk. The exposure was rendered instantaneous by passing a diaphragm with a slit in it in front of the camera, the rapidity of motion being regulated by a series of springs. Any irregularity in the motion would cause variations in shade in the photographs; but these would form bands parallel to the slit, while the shade mentioned above was not parallel to it and was curved so as to follow the moon's edge. Since, then, there is an increase both of the actinic power and of the heat, it would seem that these effects are real, since the methods of observing them are so totally different that no error in one could be introduced into the other. The only explanation of the phenomenon that seems possible is to assume the presence of a lunar atmosphere. The corona would then be caused by refraction, light reaching the observer from parts of the sun already eclipsed. Although for various reasons this hypothesis is unsatisfactory, yet it is strengthened by other observations. The protuberances have often seemed to indent the moon's edge, an appearance usually ascribed to irradiation. Several of the photographs, however, show this same effect; and in some of them the exposure was so short and the edges of the protuberances are so well defined that it cannot be caused by the intensity of their light, but must have its origin outside of the eye of the observer. It is noticeable on all sides of the moon, sometimes in half a dozen protuberances in a single photograph. An atmosphere of rapidly increasing density might produce this effect by reflection, and of course would not influence the corona if it was caused by refraction. On this supposition reliance could not be placed on measurements of the moon's diameter by occultations, or by contacts during eclipses, and would account for the uncertainty of this constant.

The principal reason for supposing the corona a portion of the sun is, that during totality it does not appear to move with the moon, but remains concentric with the sun, or, more properly, is brightest where the sun's edge is nearest. Many of the photographs show this very well, the difference on the two opposite sides of the moon being very marked. Now this effect would be explained equally well by supposing the corona caused by refraction. For the centres of the sun and moon never differ during totality by more than half a digit, while the breadth of the corona is sometimes several times as much; so that merely covering a small portion of it would not produce a greater diminution of light than would be caused by a slight change in the direction of the sun's rays shining through a lunar atmosphere. On the other hand, it is difficult to conceive of an atmosphere dense enough to produce these effects, and yet so transparent that the edges of the full moon are perfectly di-

stinct, and that the light of the sun during an eclipse should be increased rather than diminished. Again, we should expect that such variations would be produced by changes of temperature that they could scarcely fail to be detected.

We then conclude that the polariscope gives only negative results, and cannot be regarded as proving that the light is reflected. The evidence of the spectroscope needs confirmation, since the dark lines may have been invisible owing to the feeble light of the corona. But if the observations with it are correct, the self-luminous character of the corona is established. The thermometric and actinic experiments point towards a lunar atmosphere as the cause of the corona.

In the above I have endeavoured to give the evidence in favour of each view, unbiased by any theory, leaving to those best able to judge to determine whether either explains all the facts observed. The absence of a lunar atmosphere is so generally admitted, that its existence is suggested only with reluctance, and merely as the most natural explanation of the observations.

Boston, U.S., Sept. 1, 1869.

XXXIV. *Investigations on the Conformity of Vapours to* Mariotte *and* Gay-Lussac's *Law.* *By* Dr. Hermann Herwig*.

[With a Plate.]

§ 1.

THE relation which, according to the twofold law of Mariotte and Gay-Lussac, in the case of an elastic fluid connects the three quantities the pressure P, the volume V, and the absolute temperature $a+t$, cannot, after the experiments of Regnault, be considered strictly valid for permanent gases. Many important deviations from this law may be acounted for by the vapours being near their point of condensation. Very few direct experiments have been made as to the actual relation holding in the case of vapours between the quantities P, V, and $(a+t)$. More frequently has half this problem been attacked, by assuming the constancy of one of these three quantities and deducing the reciprocal dependence of the other two.

The first more nearly exact numbers were given almost simultaneously by Bineau and Cahours. Bineau† found the vapour-densities of acetic acid, of formic acid, and of sulphuric acid too high; whereupon Cahours pointed out the influence of the selection of too low temperatures by Bineau; for he showed for several bodies under a constant pressure (of one atmosphere) the mutual

* Communicated by the Author, having been read before the Niederrheinische Gesellschaft für Natur- und Heilkunde, August and November 1868. Translated by H. R. Greer, Esq., B.A.

† *Comptes Rendus*, vol. xix. p. 767.

dependence of temperature and density, *i. e.* of temperature and volume. Cahours's investigations do not justify a wider conclusion than the general one that these bodies exhibit a vapour-density more widely different from the theoretical one the nearer they are to their condensation. Bineau then furnished a few numbers concerning the relation between all three quantities, P, V, and $a+t$, for the three above-named acids. However, these few numbers demonstrate only the absolute fact of a departure of vapours from the laws of the ideal gaseous condition.

Regnault showed later* for aqueous vapour, that at low temperatures (from 30° to 55°) it does not conform to the laws of gases until the tension amounts to about ·8 of the maximum tension corresponding to the particular temperature.

More detailed investigations respecting the same vapour were instituted by Fairbairn and Tate†. These physicists determined the specific volume of perfectly saturated vapour for temperatures from 136° to 199° and from 243° to 288° Fahr., and, further, deduced the coefficient of dilatation for vapour heated some degrees above the latter temperature. Their method consists in heating different quantities of water to the same degree in two communicating globes; a change in the levels of the mercury enclosed in them indicates the moment when the smaller mass of water is changed entirely into vapour, and so a less tension commences to be exerted. But in this mode of operating there lurk many sources of error. My own experiments have above all things assured me of this, that it is by no means at the same instant when the temperature that has been reached requires theoretically a certain density that the vapour will indicate the corresponding pressure, but a certain time is requisite for the manifestation of this condition. I have found generally that the vapour does not pass instantaneously even from a superheated state into another degree of superheating as soon as the external circumstances are produced. Much more slowly will the formation of stable conditions proceed at the limit of the saturated state.

Besides this incorrectness in the method of Fairbairn and Tate, it appears also, from the arrangement of their bath, to be scarcely possible that the temperatures prevailing in the globes should be sharply defined.

Hirn also has investigated the case of aqueous vapour‡. He has calculated the volume of the (superheated) vapour under pressures of 1, 3·5, 4, and 5 atmospheres, and at a few different temperatures for each. Thus the degree of the dilatation of superheated aqueous vapour is maintained under different circum-

* *Mém. Acad. Scien.* vol. xxvi. p. 700.

† Phil. Mag. S. 4. 1861, vol. xxi. p. 230.

‡ *Théorie Mécanique de la Chaleur.*

stances. Unfortunately these experiments are not very numerous (in all about twenty).

Quite recently Horstman has published* experiments on the interdependence of the pressure, volume, and temperature of the vapours of bisulphide of carbon and of ether; but these he does not consider sufficiently trustworthy to warrant the deduction of a law from them. More correct are his experiments on the interdependence of temperature and vapour-density, under a pressure of one atmosphere, for ether, water, and acetic acid, which lead to the same result as the experiments of Cahours.

The survey of these incomplete observations shows that many experiments are still necessary in order that the problem so peculiarly interesting for the mechanical theory of heat may meet with its solution. Even for one limit of all the conditions of vapour which come into question here, viz. the case of perfect saturation, a very imperfect support has been afforded by observations to the theoretical speculations concerning the mechanical theory of heat. According to a method which I will presently describe, I have attempted to furnish some contributions to the solution of this problem.

§ 2.

The apparatus, which is intended to render a simultaneous variation of pressure, volume, and density possible, was indicated to me by Professor Wüllner, to whom I return my best thanks for the friendliness with which he always allows my work to be carried on in his laboratory.

The vapour was placed over mercury, in a divided carefully calibrated tube (*a b*, fig. 1, Plate II.), of 3·9 centims. diameter and 48 centims. length, which was firmly clamped, with its lower end open, by means of an india-rubber plug in an iron sheath. By means of a screw and a piece of caoutchouc this sheath was fastened in a cavity in a thick iron plate (*r s*), 15 centims. long and 10 broad; in this plate was a second cavity, connected with the former by an interior canal, and in which a smaller iron sheath was similarly fastened. In this last sheath there was fastened, by means of an india-rubber plug, a tube 6·8 centims. wide, 2·6 centims. long, terminating above in a narrow tube (*c d*), which served as a reservoir for the mercury which would overflow from the calibrated tube when filled with vapour. The apparatus was placed in a copper bath, 64 centims. high, 25 long, and 16 broad, in the two front sides of which were glass plates, so that both tubes were visible in their whole circumference during the observation. On the two other sides of the bath there were cases closed at the top and cut out of sheet iron; under these the heating gas-flames could be kept quite steady. The temperature of the bath

* Liebig's *Annalen*, Suppl. vol. vi. p. 51.

was indicated by fine Geissler normal thermometers graduated to the tenth part of a degree, which were controlled by comparison with other normal thermometers, and by repeatedly checking their fixed points. By means of a double stirrer, which could be rapidly moved up and down, a uniform temperature was preserved throughout the bath. Outside the bath a T-shaped glass tube, *p o m n*, was now connected with the protruding end of the tube, *c d*, by an india-rubber tube and some luting-wax. The descending branch (*o n*) of this tube, which was provided with a perforated glass cock, was connected with an air-pump, while the other end, *o m*, conducted into a chloride-of-calcium tube, *u*, and thence into a manometer, *e f*. The connexion between these last two ends was made by means of an india-rubber plug, which embraced the narrow tube and was forced into the larger one. All the points of connexion were so tightly secured that the apparatus, so long as it was in use, was perfectly air-tight, even under the highest ranges of the manometer. A barometer (*g h*) of a very wide bore gave the atmospheric pressure, whilst a thermometer (t') placed beside this and the manometer gave the corresponding temperature.

The course of investigation was as follows:—As soon as the calibrated tube, being perfectly dry, was filled with warm, very pure and dry mercury, freed to the utmost from air, and when a bursting bulb containing a weighed quantity of fluid had been placed on this, it was closed by means of a small wooden disk, lined on one side with caoutchouc and provided with a knob on the other, and being then inverted was placed in the larger iron sheath. The latter operation was rendered possible by placing about the sheath a wooden case which, filled with mercury, afforded plenty of room for the purposes of manipulation. Into the smaller iron sheath the tube *c d* was introduced half filled with mercury. The remaining half of the same, being still free, served for the reception of the mercury that overflowed from the calibrated tube in the course of the experiment, while the circumstance that the lower half already contained mercury facilitated the necessary compression. For compressions, the calibrated tube *a b*, as well as the india-rubber collar embracing it, was secured firmly to the iron sheath by means of iron rods and a cross tie. This portion of the apparatus being thus prepared was placed in the bath, and, with the principal tube in a strictly vertical position, was united, after the fashion described above, to the other part, which was fastened to a strong fixed table on which the whole stood. Now, to measure the mass of air from which such a large tube could scarcely be kept entirely free, the air in the intermediate part of the apparatus (*d p o m u e*) was greatly rarefied by means of the air-pump while the bath was kept at a given temperature; and after closing the

stopcock at n the apparatus was thus kept unchanged for some time. Hence the air collected itself over the mercury which lay deep in the tube $a\,b$; and when this had been effected, the tightness of all the communications of the apparatus could be simultaneously controlled.

Then by varying the pressure of the air in the intermediate part of the apparatus, the volume of the air confined in the tube $a\,b$ was made to vary, and that from the largest to the smallest possible volume, while the simultaneous states of pressure and volume were, naturally, measured with the bath at constant temperature. To determine the pressure there were six mercury-levels to be measured—besides those of the barometer and manometer, those in the tubes $a\,b$ and $c\,d$. A very excellent cathetometer with a corrected telescope, by Staudiger of Giessen, which admitted of reading off to the tenth part of a millimetre, was used for this purpose. From one set of determinations of the simultaneous pressure and volume of the enclosed air, the quantity itself was determined with perfect accuracy. They could also be applied to the purposes of direct calculation in afterwards measuring the total tension exerted in the tube $a\,b$; this, however, was never very great.

Now the bulb filled with liquid was burst, and to obtain the solution of the real problem, viz. the determination of the volume, pressure, and density of vapour formed under different circumstances, we proceeded as follows. The relation between pressure and volume, always at a constant temperature, was to be sought from the point of saturation of the vapour up to the point where, for this temperature, it follows Mariotte's law; and different temperatures would be investigated in this wise. For this purpose, first of all, a definite temperature of the bath was maintained with the greatest care, which could be effected very readily by reason of the large size of the bath (25 litres) and the mode of heating employed, which was scarcely disturbed by draughts. It was possible to maintain the temperature invariable within 0·1 of a degree for a series of hours, and during the time of measurement to keep it steady to ·05 of a degree. The temperature being constant, then, as in the measurement of the air, as large a volume as possible of vapour was produced, and made to pass thence into a smaller volume by means of the gradual introduction of air into the intermediate part of the apparatus. However, before taking a measurement of the coexisting pressure and volume, a considerable pause was made each time so as to allow the condition of the vapour to become stationary. The commencement of the stationary condition could be recognized by the repeated measurements.

We may remark that the converse process (of passing to a larger volume from the state of saturation of the vapour by

means of a gradual rarefaction of the air in the intermediate part of the apparatus) does not recommend itself. We should then run the risk of individual particles of fluid adhering to the glass, without evaporating, perhaps much longer than would correspond with the particular temperature and rarefaction of the air. However, before any measurement was taken, we kept the vapour for a long time dilated to such a volume that it obeyed Mariotte's law at the defined temperature, and then allowed it to proceed to a smaller volume by the gradual introduction of the air, whereby a longer time was allowed for the acquisition of a constant condition before each measurement of the vapour, so that we had more confidence that we were observing circumstances which actually corresponded to the external pressure and temperature.

The determination of the pressure by the measurement of the six mercury-levels could be made very accurately with the above-named cathetometer. The cathetometer itself, which stood on a strong fixed table, was daily corrected.

Through the telescope of the cathetometer we could clearly read off the volume of the vapour found in the calibrated tube to the tenth part of a cubic centimetre. Having measured the volume and pressure coexisting at the given temperature, we then subtracted from the latter the pressure exercised under these circumstances by the air-bubble, which had been determined first of all. For each temperature, the volume v and the pressure p of the vapour were measured from the maximum of tension, *i. e.* from the saturation of the vapour, to such a distance from saturation that the vapour followed Mariotte's law. The commencement of this latter was manifested by the constancy of the product pv, which up to this time had been always increasing.

§ 3.

One word here as to the accuracy of the numbers thus arrived at. Neither the apparatus nor the method of investigation can admit of errors from any other source than the two usual ones, which cannot be quite avoided, viz. slight variations of temperature in the bath, and slight irregularities in placing the cathetometer on the six quicksilver-levels. As to the first, it has been already remarked that the variations of temperature arising during the measurement did not amount to ·05 of a degree. The error arising hence in the estimation of the tension (which was not necessarily in strict accordance with the same temperature, yet at most could vary from the specified temperature on either side to the extent of 0·5 of a degree) is greater or smaller as the variation of the tension with the temperature is greater or smaller. The extreme case must be that of the maximum tension. Taking the maximum tension of alcohol at 69° as 537·63, a variation of temperature of 0°·05 would correspond to about 1 millim. However, that the errors which actually occurred never reached these amounts

is shown by a mutual comparison of the maximum tensions at different temperatures. To the sum of the errors in tension is still to be added the influence of the second of the above-named circumstances, viz. the variation in the position of the cathetometer when placed successively on the six quicksilver-levels, which cannot have been of precisely similar form in all respects. But in general we found under the maximum tension a deviation of only 0·5 of a millim. from the mean; the greatest deviation that occurred is ·6 of a millim. in the case of alcohol at 62°·9, where the mean of eight measurements of maximum tension amounted to 396·83 millims., while the measurement in which the aberration was greatest was 397·43.

From a variation in adjusting the cathetometer on the mercury-level in the tube containing the vapour, and from placing the tube in a position not exactly vertical, a further error in taking the volume might be committed, to the amount, perhaps, of 0·3 of a cubic centimetre. In order to check the errors arising from this source, we had to see how much one of the products $p\,v$, which for any one temperature already obeyed Mariotte's law and were constant, deviated from the mean of all these $p\,v$'s, and, moreover, how widely this mean deviated from the mean values holding for other temperatures, differently from what is required by Gay-Lussac's law. We had also to take the mean of the vapour-densities for the different temperatures which are derived from the constant $p\,v$ of each temperature, and calculate accordingly the true mean values of the constants $p\,v$ for each temperature, and then seek for the greatest deviation therefrom. Besides these errors in volume, the errors in tension already spoken of would also naturally come into consideration. But we invariably found much smaller deviations than the extreme deviation, which arises in the case of alcohol at 69°·9, where, with a volume of 93 cubic centims. and a tension of 127·54 millims., the product 11861 was calculated instead of the true mean value 11797. If we here assume an error of ·3 cubic centim. in volume, the additional error in tension will only amount to 0·3 millim., which is far within the specified limits. Upon the whole it follows, then, that the greatest errors in tension are to be taken at most at 0·6 millim., and of volume at ·3 cubic centim., and that these limits were reached in very exceptional cases only.

§ 4. *Examination of the Vapour of Alcohol.*

The first numbers found, according to the method sketched out, were those given in the following Table for alcohol. They contain the values of the volume v (in cubic centims.) and of the tension p (in millimetres of mercury) corresponding to the eight temperatures examined. There are also given the products $p\,v$. The cessation of saturation, as well as the occurrence of Mariotte's law, is indicated on each occasion by the horizontal lines.

TABLE I.—Alcohol.

23°.			30°·5.			36°·4.			41°·9.		
v.	*p.*	*pv.*	*v.*	*p.*	*pv.*	*v.*	*p.*	*pv.*	*v.*	*p.*	*pv.*
151·4	50·42		42·1	77·75		40·4	107·87		14·8	144·78	
164·9	50·41		58·7	77·61		56·5	108·27		22·7	144·81	
182·3	49·72		70·3	77·30		72·5	107·84		31·4	144·50	
188·2	50·33		83·3	77·85		89·6	108·04		39	144·95	
191·3	50·29		99·9	77·15		95·2	107·26	10207	49	144·67	
197·8	50·23		119·5	77·67		97·9	106·68	10448	55	144·75	
201·6	49·83	10046	125·9	77·74		101·1	104·40	10555	68·7	144·38	
203·4	49·58	10085	133·2	76·93	10247	101·4	104·07	10553	69·9	144·75	
203·8	49·53	10094	136·8	76·09	10409	105·9	100·32	10624	73·6	143·36	10553
205·7	49·32	10144	138·95	74·91	10409	111·3	95·30	10607	76	141·23	10719
207·7	48·87	10153	139·8	74·42	10404	133·3	79·56	10606	78·1	137·56	10733
213	47·77	10174	143·4	72·71	10427	143·2	74·26	10635	81·4	132·72	10806
219·7	46·48	10210	147	70·78	10404	161·8	65·83	10653	85·1	127·40	10841
232	44·05	10221	158·2	66·03	10446				89	121·71	10831
238·4	42·76	10196	171·7	60·83	10445				94·85	114·64	10875
									99·3	109·42	10864
									129·5	83·70	10839
									139·9	77·38	10828
									147·5	73·63	10861
									153·85	70·69	10876

47°·8.			57°·8.			62°·9.			69°·9.		
v.	*p.*	*pv.*	*v.*	*p.*	*pv.*	*v.*	*p.*	*pv.*	*v.*	*p.*	*pv.*
38	196·48		31	315·66		14·7	396·79		14·3	538·18	
38·8	196·51		31	316		16·6	397·43		16·8	537·37	
42·2	196·44		31·7	315·77		17·1	397·18		19·8	537·35	
42·3	196·37		32·8	315·77		17·8	396·88		22·7	504·01	11441
42·8	196·77		33	315·81		18·4	396·39		26	441·31	11489
46	196·60		35	311·63	10913	20·3	396·31		29·2	394·54	11531
52·5	196·34		38·7	289·03	11191	20·7	396·92		31	373·32	11582
54·4	195·81	10654	38·7	289·13	11195	25·2	397·19		35	332·21	11634
55·6	195·08	10849	43·7	257·13	11241	27·5	396·21	10895	40·8	287·02	11717
58	188·08	10909	50	225·79	11292	28·4	392·84	11169	45·5	258·02	11744
61	179·43	10945	56	201·98	11313	28·7	389·88	11202	51	230·86	11776
64·7	169·32	10955	60·7	186·64	11329	32·7	345·19	11295	54·6	216·44	11821
68·8	159·49	10973	65·7	173·06	11372	37·5	302·84	11363	55·5	212·53	11798
73·3	150·48	11030	74	154	11397	42·6	268·05	11424	60·6	195·27	11833
73·3	150·27	11015	80·7	141·01	11381	47·1	242·97	11446	65·7	180·28	11845
79·2	139·24	11026	91	125·42	11411	51	225·49	11499	70·6	167·63	11835
79·2	139·11	11017	94·5	120·24	11358	58	198·72	11526	76·6	154·46	11832
86·2	128	11033	104·1	109·83	11434	65	177·44	11534	85·3	139·04	11860
86·4	127·60	11024	122	93·28	11382	70·5	164·05	11566	93	127·54	11861
96·55	114·38	11046				75·2	153·85	11570	102·1	115·56	11799
107·2	102·89	11030				79·5	145·22	11544	108·3	108·74	11777
112·2	98·64	11066									
112·5	98·32	11060									
120·5	91·78	11062									
131·8	83·82	11047									

The size of the air-bubble which was present amounted to ·064 cubic centim. for 0° and a pressure of 0·760 millim. The weight of the alcohol examined was ·0248 grm. Hence are calculated, for the following different temperatures, the final vapour-densities which correspond to the mean value of the constant pv for each temperature.

Temperature ...	23	30·5	36·4	41·9	47·8	57·8	62·9	69·9
Vapour-density .	1·550	1·555	1·555	1·550	1·552	1·551	1·552	1·548

That these densities are all too small is due simply to this—that the alcohol that was used was not entirely free from water, but had been allowed to stand in the air for a considerable time in a flask closed by only a cork. On this the first filling of the apparatus, it was my intention only to test its accuracy. However, as it immediately proved itself to be reliable, I then carried on this first investigation to the end. But even as regards the object in view, it is of small consequence whether the alcohol were perfectly pure or contained some water; it is only necessary to keep in mind that the numbers obtained above refer to alcohol not entirely free from water.

A comparison of the vapour-densities obtained at different temperatures shows clearly that the vapour-densities are constant. It therefore exhibits the simultaneous appearance of Gay-Lussac's law and that of Mariotte; and, indeed, nothing different could have been expected *à priori*. At the same time it is shown experimentally that by means of the apparatus here employed the vapour-densities can be accurately determined even at low temperatures (much below the boiling-point of the bodies examined), which is worth noting, by reason of the difficulty encountered in the determination of the vapour-densities of several bodies when at a high temperature according to the usual methods.

A further comparison of the figures entered in column pv, the particulars of which exhibit the magnitude of the deviation of the vapour from Mariotte's law at different temperatures, shows us that at each approach to condensation the deviation increases with ascending temperatures. That it does so in the case of water, at least, Clausius tells us in his first memoir*.

If the volume and density of perfectly saturated vapour, which thus has absorbed the last drop of liquid, be denoted by v_1 and p_1, while V and P are the corresponding quantities for a condition of the vapour in which it already obeys Mariotte's law at the specified temperature, then the quotient $\frac{PV}{p_1v_1}$ will increase with increasing temperatures.

* The Mechanical Theory of Heat. London, 1867. Van Voorst.

Furthermore we may also see from the above numbers an increase of the product p_1v_1 with an increasing temperature. Put $\frac{\mathrm{PV}}{p_1v_1}=f(t)$ and $p_1v_1=\phi(t)$; then we shall have $f(t)$ and $\phi(t)$ functions of the temperature t, and increasing with it. The product of these functions, $f(t) \cdot \phi(t)$, or PV, must be a function of the temperature such that $\mathrm{PV}=\text{const.}\ (a+t)$, if by $(a+t)$ the absolute temperature is denoted. This relation, as well as the proportionate mode of increase of both the functions $f(t)$ and $\phi(t)$ when taken at all possible magnitudes, led me to the conjecture that perhaps the assumption $f(t)=c\sqrt{a+t}$ and $\phi(t)=c_1\sqrt{a+t}$, where c and c_1 are constant, might fall in with the numbers found. In order to prove this, in the first place I selected some of the observed temperatures in which I had seen with tolerable precision the point of cessation of maximum tension (*i. e.* I knew the value of v_1), and calculated therefrom, as the value of c, $c=\cdot059487$, on the assumption $f(t)=c\sqrt{a+t}$. With these values I then calculated the value of v_1 for the other temperatures, where I had not so accurately observed the limit of the maximum tension. The following Table contains the values of v_1, as well as the two members of the calculation.

Table I. *a.*

Temperature t	23°	30°·5	36°·4	41°·9	47°·8	57°·8	62°·9	69°·9
Mean of the observed PV	10191	10421	10625	10852	11038	11391	11554	11826
Mean PV corrected for the mean vapour-density 1·552	10183	10442	10644	10834	11038	11381	11554	11797
$\sqrt{a+t}$	17·205	17·421	17·590	17·745	17·911	18·188	18·328	18·518
$0\cdot0595\sqrt{a+t}=\left(\frac{\mathrm{PV}}{p_1v_1}\right)$	1·02347	1·03632	1·04638	1·05560	1·06547	1·08195	1·09028	1·10158
p_1v_1 calculated from this by the aid of PV.	9949	10076	10172	10263	10359	10519	10597	10709
p_1, mean of the observations................	50·23	77·58	108·00	144·70	196·50	315·80	396·83	537·63
v_1 calculated from this.	198·1	129·9	94·2	70·9	52·7	33·3	26·7	19·9

Since the accurate determination of v_1 can hardly be made in this way by experiment, because the tension recedes so slowly from the maximum that the differences of the tension in the neighbourhood of the real v_1 lie within the errors of observation, and since in the investigation of alcohol I had not so carefully noticed the cessation of the maximum tension, I give therefore, as follows, the extreme limits between which v_1 must always fall without directly contradicting the observations; also I have calculated for these limits the values of c in the formula $f(t)=c\sqrt{a+t}$.

TABLE I. *b*.

Temperature t.	23°	30°·5	36°·4	41°·9	47°·8	57°·8	62°·9	69°·9
Limits of v_1	197·8	125·9	89·6	69·9	52·5	33	25·2	19·8
	201·6	133	95	72	54·4	34·2	27·5	20·5
Corresponding c......	0·05957	0·06137	0·06247	0·06036	0·05974	0·06004	0·06304	0·05977
	0·05613	0·05809	0·05898	0·05860	0·05765	0·05793	0·05777	0·05773

A consideration of these figures gives great probability to the assumption that in c we have a genuine constant; and comparing the v_1 calculated with $c = \cdot0595$, as above, with the particulars of the tension in the neighbourhood of these volumes, as they may be seen in Table I., it would appear with the highest probability that these values are correct. Hence it appears to me that the relation $\frac{PV}{p_1v_1} = \cdot0595\sqrt{a+t}$ holds actually, at least for such temperatures of alcohol-vapour as have been examined. Taking this relation as universally correct for alcohol, it follows hence that, for the particular temperature at which $\cdot0595\sqrt{a+t} = 1$, the product $p_1v_1 = PV$; *i. e.* that at this temperature the vapour of alcohol, so soon as it is separated from the fluid, already follows Mariotte's law. As to the temperature at which this happens, it is calculated from the value $c = \cdot059487$ as $t = 9°{\cdot}589$ Celsius. The investigation, unfortunately, could not be carried on as far as this temperature in the warm weather of the season; ice thrown into the bath would not have given sufficiently steady temperatures. Moreover the deviation of the vapour from Mariotte's law which exists at 23° is already so small, that it only slightly oversteps the possible errors of observation in the slight tension belonging to that temperature. Now, whether the relation $f(t) = \cdot0595\sqrt{a+t}$ holds good quite to the temperature of 9°·5 for vapour of alcohol, and whether at that and lower temperatures the vapour follows Mariotte's law when free from fluidity, or whether a slight departure from Mariotte's law takes place in the opposite direction (perhaps even according to the law $f(t) = c\sqrt{a+t}$), just as Regnault found for hydrogen under a high pressure*, is a question which must be decided by further investigations, attended, of course, by greater difficulties; and these I intend to execute.

With respect to the particulars of the products pv which lie between p_1v_1 and PV, after many trials I have not been able to find any formula to which these products would conform as to an actual law. It is probable that the relation actually existing for these products is complicated, like the tension-curve of saturated vapours, the theoretical expression for which has hitherto been sought in vain.

* *Mém. de l'Acad. des Sciences,* vol. xxi. p. 395.

§ 5. *Examination of the Vapour of Chloroform.*

As the second fluid I took chloroform; during the examination of this, in order to avoid the chemical influence of light, I covered the side of the bath which was turned towards the window with a piece of yellow glass. For this preparation, as well as for the bisulphide of carbon, which will be discussed further on, both of them perfectly pure, I return my best thanks to Dr. Glaser. The following Table gives the numbers for chloroform, obtained in the same manner as those given for alcohol:—

TABLE II.—Chloroform.

30°·4.			39°·8.			49°·8.			64°·8.		
v.	*p.*	*pv.*	*v.*	*p.*	*pv.*	*v.*	*p.*	*pv.*	*v.*	*p.*	*pv.*
29·3	243·21		35·6	354·58		21·2	514·13				
48·1	243·19		38	354·98		28·2	514·25				
55·7	242·92		48·4	354·76		35·6	514·14		27·4	843·75	23144
61·3	243·19		57·5	354·67		42·1	513·76		34·4	687·31	23664
70·7	243·21		60·5	354·86		—	—	—	40·5	588·72	23856
74·7	243·08		—	—	—	45	504·74	22730	40·8	584·78	23871
83·5	242·78		65·4	332·73	21761	51·7	443·84	22946	47·4	505·42	23962
87·2	242·99		70·6	314·08	22175	58·9	390·70	23016	57	421·49	24025
—	—	—	77	290·57	22372	65·7	351·07	23065	63	382·92	24124
91	238·56	21709	83	269·94	22403	74·2	311·77	23133	70·7	341·58	24149
92·7	234·55	21738	91·6	244·96	22434	83·7	276·63	23151	83·3	290·98	24236
98·9	220·09	21776	98·6	227·66	22452	91·4	253·75	23188	92·7	261·69	24254
104·8	208·10	21809	103·8	216·99	22524	97·7	237·40	23193	100·8	240·94	24295
112·4	194·53	21862	—	—	—	101·6	228·43	23215	100·9	240·74	24304
120	182·44	21899	109·5	206·14	22572	108·4	214·58	23260	111·2	218·69	24315
—	—	—	125·6	179·75	22572	117	198·98	23271	113·1	215·13	24327
132·5	165·54	21930	129·9	173·90	22589	—	—	—	116	209·96	24347
140·7	155·70	21908	130	173·60	22567	118·6	196·44	23306	122	199·90	24393
141·3	155·25	21931	141·2	160·33	22644	124	188·05	23319	—	—	—
						133·1	175·25	23321	128	190·95	24442
						145·4	160·29	23307	128·3	190·37	24423
									128·6	189·79	24407
									136·5	179·15	24447
									140·8	173·64	24450

The air-bubble amounted in this case to 0·31 cubic centim. at 0° and under a pressure of 760 millims. The weight of the chloroform examined was ·1406 grm. The final constant vapour-densities calculated therefrom are, for the different temperatures, the following, which agree sufficiently:—

Temperature	30°·4	39°·8	49°·8	64°·8
Vapour-density	4·190	4·191	4·191	4·185

These vapour-densities differ more from the theoretical one (4·138) than can be accounted for by small errors in weighing. Indeed I think I remarked for some hours (before the beginning

of the measurements), when the chloroform was not yet protected by the yellow glass, that a small trace had been already decomposed. This, however, could not make a greater difference in the weight than 1·5 milligrm. The examination of bisulphide of carbon, which will be subsequently described, gave a similar result, where the traces of the sulphur which might be separated during the boiling out and sealing of the bursting bulb also could not have produced the difference of weight necessary in order to bring the vapour-densities actually found into accordance with the theoretical ones. That in both these cases no error can lurk in the method which would induce the differences may be indubitably recognized from this, viz. that at each temperature the final vapour-densities for the most various volumes are, within the limits of errors of observation, exactly proportional to the final vapour-densities at all other temperatures. Besides, in general the experimental determinations of the vapour-densities do not rigorously lead to the theoretical densities. Even though many of the old determinations could not give any exact results because no attention was paid to the question whether the vapours were sufficiently far from their condensation, yet deviations from these causes must always give only an increase in the vapour-density over the theoretical values, while a converse course of determinations would furnish equally important smaller values.

Now, as to the relation holding for vapour of chloroform corresponding to that found for alcohol, viz. $\frac{PV}{p_1 v_1}=c\sqrt{a+t}$, I first of all conjectured that, even if the like holds here also, the constant c might perhaps be different from that found to be valid for chloroform, in such sort that the temperature at which the perfectly saturated vapour follows Mariotte's law might, for chloroform, lie as much under $9^\circ{\cdot}5$ as the boiling-point of chloroform under atmospheric pressure lies under the boiling-point of alcohol. Meanwhile the first set of experiments showed decisively that this was not the case; on the other hand, the surprising result presented itself, that in the admittedly valid formula $\frac{PV}{p_1 v_1}=c\sqrt{a+t}$ the constant c had the same value as for alcohol. In what follows I give the Table of v_1 calculated from the specified relation with $c=\cdot0595$, and at the same time, as for alcohol, the extreme limits of v_1 and c_1 which are consistent with the observations. In this case I have sought to observe more accurately the exact point of retreat of the vapour from the state of maximum tension. I must remark that at the last temperature ($64^\circ{\cdot}8$) the apparatus unfortunately did not sustain the compression which was necessary in order to arrive at the state of maximum tension. The only observations that I could make

with certainty at 64°·8 are those given in Table II. But by the help of one approximately estimated maximum tension, which is taken from the relation of the remaining maximum tensions to those of Regnault* (touching which I may remark that the difference between the two is greatly affected by the difference in the preparations), the probably correct value of v_1 may be calculated, since with a small value of v_1 and a high value of p_1 a mistake in the latter to the amount of a few millimetres would alter the value of v_1 only very little.

TABLE II. *a*.

Temperature t	30°·4	39°·8	49°·8	64°·8
Mean of the observed PV...	21923	22590	23313	24434
Mean PV corrected for the mean vapour-density 4·189	21928	22602	23313	24399
$0·0595\ \sqrt{a+t}\left(=\frac{PV}{p_1v_1}\right)$......	1·03614	1·05209	1·06881	1·09331
p_1v_1 calculated from this by the aid of PV corrected	21164	21483	21812	22317
p_1, mean of the observations.	243·08	354·77	514·07	870 nearly
v_1 calculated from this	87·1	60·6	42·4	25·7

Table II. *b*.

Temperature t	30°·4.	39°·8.	49°·8.
Extreme limits ${}_1$	87·1 88·5	60·5 61·5	42·1 43
Corresponding c in the ratio $\frac{PV}{p_1v_1}=c\sqrt{a+t}$ ...	0·05949 0·05858	0·05961 0·05864	0·05997 0·05871

A survey of these Tables shows how closely the assumption $\frac{PV}{p_1v_1}=0·0595\sqrt{a+t}$ harmonizes with the observations. Hence also for alcohol and chloroform the same temperature (9°·5) must exist at which the vapours of both fluids, so soon as they pass from a fluid state, follow Mariotte's law; the point of maximum tension (very different at 9°·5) appears to have no influence on the position of this temperature.

§ 6. *Examination of the Vapour of Bisulphide of Carbon.*

To prove perhaps the universal validity of this remarkable phenomenon, there was taken for the third body bisulphide of carbon, the maximum tension of which at 9°·5 is considerably greater than that of chloroform. This body, having been prepared so as to be quite pure, was protected from the light during the investigation by a piece of yellow glass. The following Table gives the simultaneous v and p for five temperatures, and therewith the respective products pv.

* *Mém. de l'Acad. des Sciences,* vol. xxvi. p. 403.

TABLE III.—Bisulphide of Carbon.

8°·5.			14°·2.			20°·1.			32°.			35°·9.		
v.	*p.*	*pv.*	*v.*	*p.*	*pv.*	*v.*	*p.*	*pv.*	*v.*	*p.*	*pv.*	*v.*	*p.*	*pv.*
55·6	183·39		41·7	234·60		14	294·37		13·9	461·92		21·1	531·28	
69·8	183·00		51·2	234·58		20	294·02		28·1	461·35		23·2	531·48	
85	182·88		63·1	234·65		23·5	294·43		32·3	461·30		24·6	531·73	
91·3	176·75	16134	63·3	234·12		24·7	294·30		34·7	461·58		27·1	531·57	
121	133·75	16191	69·2	234·28		36·4	293·81		37·6	455·94	17153	31·4	531·91	
134·5	120·13	16153	71·9	227·73	16374	45·8	294·57		42·7	402·56	17198	34	509·79	17343
140·8	115·19	16223	72·1	227·34	16392	53·9	293·97		52·2	331·17	17291	44·8	389·95	17474
			72·1	227·20	16381	54·5	293·90		61·8	280·87	17358	55·6	315·54	17547
			72·3	226·79	16397	55·1	294·02		81·6	213·49	17420	66·3	265·34	17592
			81·2	202·29	16424	56·2	293·86		100·7	173·32	17459	83·7	211·11	17668
			92·2	178·66	16468	60·2	276·90	16670	110·6	158·13	17488	99·4	178·01	17699
			105·8	155·97	16502	77·6	215·07	16689	125·9	139·78	17599	114·9	154·43	17738
			119·4	138·44	16535	86·8	192·70	16719	141·6	124·09	17575	126·7	140·50	17801
			129·2	127·97	16533	92·3	181·28	16728	155·95	112·48	17541	132·2	134·42	17768
			133·4	123·74	16503	102·5	163·88	16801	163·1	107·58	17547	147·3	121	17824
			137·55	120·05	16513	122·4	137·45	16827				159·8	111·22	17777
			141·4	116·87	[illegible]	129	130·85	16880				166·1	107·12	17792
			143·9	114·89	16534	131·9	127·82	16859						
						138·3	121·98	16875						
						145	116·50	16893						

The size of the air-bubble was ·325 cubic centim. at 0° and 760 millims. pressure; the weight of the bisulphide of carbon was ·0717 grm. The final vapour-densities calculated therefrom agree with one another very well, as the following comparison shows:—

Temperature	8°·5	14°·2	20°·1	32°	35°·9
Vapour-density ...	2·686	2·683	2·682	2·680	2·680

Their deviation from the theoretical vapour-densities has been mentioned above. The relation $\frac{PV}{p_1 v_1}=c\sqrt{a+t}$ is exhibited with the same constant, $c=·0595$, in fact confirmed here also, as a comparison of the following Tables with the corresponding preceding one shows:—

TABLE III. *a.*

Temperature t	8°·5	14°·2	20°·1	32°	35°·9
Mean of the observed PV ...	16175	16521	16867	17565	17792
Mean PV corrected for the mean vapour-density 2·682	16199	16528	16867	17552	17777
$0·0595\sqrt{a+t}\left(=\frac{PV}{p_1v_1}\right)$......	1	1·00813	1·01842	1·03888	1·04554
p_1v_1 calculated from this by the aid of PV corrected	16199	16385	16562	16895	17003
p_1, mean of the observations.	183·09	234·45	294·12	461·54	531·59
v_1 calculated from this	88·5	69·9	56·3	36·6	32

TABLE III. *b.*

Temperature t	8°·5	14°·2	20°·1	32°	35°·9
Extreme limits of v_1	85 88·5	69·2 70	56·2 57	34·7 37	31·4 32·5
Corresponding c in the ratio $\frac{PV}{p_1v_1}=c\sqrt{a+t}$...	0·06204 0·05949	0·06012 0·05943	0·05961 0·05877	0·06204 0·05818	0·06057 0·05852

The temperature 8°·5 was taken on a cold October day, yet could not be maintained without the help of some ice. This certainly influenced the degree of constancy in the temperature which existed in all other cases. The numbers obtained are less accurate; but they appear to show pretty clearly that for these temperatures the vapour already follows Mariotte's law immediately on its declension from the point of maximum tension. The differences of the four products, given in Table III., are somewhat irregular; but when we take into account the small fluctuations of temperature which certainly did exist, they may lie quite within the errors of observation.

§ 7.

Now we have the relation $\frac{PV}{p_1 v_1} = c\sqrt{a+t}$ true (at least within the limit of errors of observation) for the vapours of the very different bodies treated above, viz. alcohol, chloroform, and bisulphide of carbon, the same value in all cases being assigned to the constant, viz. $c = \cdot 0595$. Simultaneously with this there must hold the other necessary relation, viz. $p_1 v_1 = c_1 \sqrt{a+t}$, where c_1 means a constant depending only on the density of each vapour, namely $c_1 = \frac{PV}{\cdot 0595\,(a+t)}$. At the same time it follows that the pressure of perfectly saturated vapour is proportional to the square root of the absolute temperature. From the relation $p_1 v_1 = c\sqrt{a+t}$, a more perfect understanding is afforded concerning the condition of perfect saturation, *i. e.* concerning one limiting condition of these superheated vapours, so soon as the curve of maximum tension is known. That important curve in the mechanical theory of heat may then be constructed which exhibits the mutual connexion of the magnitudes denoted above by p_1 and v_1, provided we know the relation existing between p_1 and t, i. e. when the curve of tension is given.

The values of v_1 given in Tables I. *a*, II. *a*, III. *a* are taken from the mass of vapour used on each occasion. Dividing these values by the weight of the vapour in question, we shall obtain the specific volumes, so called in the mechanical theory of heat, of perfectly saturated vapour. With a kilogramme as the unit of weight, we find the following specific volumes expressed in cubic centimetres :

Table IV.

Alcohol.

Temperature ...	23°	30°·5	36°·4	41°·9	47°·8	57°·8	62°·9	69°·9
Specific volumes.	7·9879	5·2379	3·7984	2·8589	2·1250	1·3428	1·0766	0·8024

Chloroform.

Temperature	30°·4	39°·8	49°·8	64°·8
Specific volumes ..	0·6202	0·4310	0·3016	0·1828

Bisulphide of Carbon.

Temperature	8°·5	14°·2	20°·1	32°	35°·9
Specific volumes ..	1·2343	0·9749	0·7852	0·5105	0·4463

The specific volumes of the three vapours examined are found, according to the principles of the mechanical theory of heat, laid down in Zeuner's *Grundzüge*, 1866, where the values of u given in Tables 3. b, 5. b, and 7. b, multiplied by the values of σ given page 288, determine the specific volumes for each vapour. The values of u given there are calculated on the basis of Regnault's formulæ for tension and the heats of evaporation communicated by Regnault. In order to render possible a comparison of the specific volumes of perfectly saturated vapour thus obtained with those derived from the relation $p_1 v_1 = \text{const.} \sqrt{a+t}$, I have calculated these last from Regnault's tensions, using the theoretical vapour-densities, and at the same time I have given the volumes which result according to the old view when the vapours are supposed to follow the laws of Mariotte and Gay-Lussac. The foundations for these last have thus been built upon Regnault's tensions and the theoretical vapour-densities.

Table V.—Specific volumes of perfectly saturated vapour.

Temperature.	*a.* According to Zeuner.	*b.* Calculated from $p_1 v_1 = c_1 \sqrt{a+t}$.	*c.* On the old assumption.
Alcohol.			
10°	17·3294	15·7730	15·7859
30	5·7316	5·0354	5·2142
50	2·1345	1·8567	1·9851
70	0·8822	0·7775	0·8566
100	0·2863	0·2585	0·2970
120	0·1538	0·1394	0·1643
140	0·0902	0·0814	0·0984
Chloroform.			
10	1·4698	1·4644	1·4656
30	0·6413	0·6150	0·6368
50	0·3143	0·2938	0·3141
70	0·1693	0·1554	0·1712
100	0·0774	0·0696	0·0799
120	0·0498	0·0442	0·0521
140	0·0339	0·0296	0·0358
Bisulphide of Carbon.			
10	1·1719	1·1660	1·1669
30	0·5662	0·5508	0·5703
50	0·3005	0·2884	0·3084
70	0·1720	0·1641	0·1808
100	0·0834	0·0799	0·0918
120	0·0547	0·0530	0·0625
140	0·0374	0·0368	0·0444

The difference between the figures entered in columns *a* and *b* is manifest. We may remark that it seems impossible that the volumes as calculated by Zeuner should be correct, as being larger than those entered under column *c*. Since, however, the figures in columns *b* and *c* are derived from the theoretical vapour-densities, possibly the preparations examined by Regnault may have possessed a somewhat different final vapour-density, which would account for the discrepancy. In the first figures the difference (in the case of alcohol) is, of course, too large to be accounted for by this explanation. Besides, we may suppose that the specific volumes calculated on the basis of the mechanical theory of heat are obtained in such a circumstantial way that such a variation in the fundamental data of observation (the maximum tension, the total amount of heat, and the heat of the fluid) as, according to Regnault's investigations, would lie within the errors of observation might evoke a proportionately important variation in the final result. For these reasons no very trustworthy conclusions can be drawn from the comparison given above in Table V.

§ 8.

As to the whole question of the superheated state of vapour, I have already remarked that as yet I have not been so fortunate as to derive a precise law from the course of the products *pv* which lie between saturation and the gaseous condition.

But some interesting conclusions may be obtained from the examination of the other limit, which separates the condition of vapour deviating from the laws of the ideal gas (which for shortness I will designate exclusively as the superheated condition) from the gaseous condition. Of course the experiment of fixing these limits accurately has never been made with success, since the product *pv* varies too slightly in the vicinity of its constant condition to allow of the differences falling without the errors of observation. Generally, from the occurrence of Mariotte's law (*i. e.* from the constancy *pv* for each temperature), we can only infer that the eventual small differences of *pv* are not perceptible by our instrumental measurements. But, on the other hand, if the examination always shows an undoubted fluctuation beyond these limits, then a further determination can be instituted to at least some degree of approximation. And in the case before us the observations suffice to exhibit a very unexpected result.

Considering now the particulars of V_1 (*i. e.* of the volumes for which the vapour first enters into the gaseous condition at different temperatures), Table II. shows for chloroform, and, still more, Table III. for bisulphide of carbon, that these volumes do not constantly diminish with increasing temperatures, as one might have supposed, but from a certain point increase with the

temperature. It is only necessary, by means of a comparison of the pv's which stand in Tables II. and III. immediately over and under the last corresponding horizontal lines with the mean volumes of PV given in Tables II. *a* and III. *a*, to make sure of the correct position of the horizontal lines, and to inspect the values of the volumes v between which the horizontal lines lie. In the case of chloroform, at the temperatures examined, may be remarked first a decrease and then an increase of V_1, while in the case of bisulphide of carbon there is a continual increase from the lowest temperature (8°·5). In the case of alcohol, nothing analogous can be seen at the temperatures examined. For the clearer exhibition of these relations, a graphic construction of V_1 may be contrived. It is usual in the plane coordinate system to take the absolute temperatures as the abscissæ x, and as the ordinates y to take simultaneously the corresponding values of the specific volumes (marked v_1) of perfectly saturated vapour, and also those of the volumes V_1 referred to the vapour-unit of weight (1 kilog.). Of the two curves so constructed, that of v_1 must always drop as the abscissæ increase, and run asymptotically into coincidence with a line parallel to the axis of abscissæ. The volume characterized by this parallel is the least at which the unit weight of vapour can exist without assuming the fluid form. For the three vapours which have been discussed, the curve of V_1 coincides with the curve of v_1 at the abscissa $= a + 9{\cdot}5$, and afterwards, with increasing values, assumes a course more removed from the axis of abscissæ than the curve of v_1; indeed it appears from all the foregoing observations, that the difference $V_1 - v_1$ constantly increases. Therefrom results the necessity of a minimum of the curve V_1, provided there be an initial descent. For chloroform this minimum is actually proved from the observations. For bisulphide of carbon the same lies close to the neighbourhood of a temperature of 10°, and then the observed constant increase of V_1 along with the temperature commences. For alcohol, the temperature at which the minimum exists, according to this, would lie higher than 70°. At higher temperatures there would probably be found a constant but small increase of V_1; at least such appears to be the most natural supposition, after the proof of a minimum of the curve V_1.

Since any volume of the unit of weight of vapour corresponds to any temperature of the superheated condition when it falls between curves V_1 and v_1 in the representation by coordinates, and, on the other hand, corresponds to the gaseous condition when it has both curves between it and the axis of abscissæ, the preceding considerations lead to the following result. We can draw a parallel (M N, fig. 2) to the axis of abscissæ from any point of the

curve v_1 which will cut the curve V_1 twice; *i. e.* the unit of weight of vapour being enclosed in an invariable volume, may at any temperature be perfectly saturated vapour; with an increasing temperature it will withdraw itself from the superheated and approach to the gaseous condition; in this latter it continues for a time under a still rising temperature, and under a higher increase of temperature it again arrives at the superheated condition, indeed probably approaches this the more nearly the higher the temperature is raised. Since, according to the mechanical theory of heat, the temperature represents the measure of the *vis viva* of molecular motion, while the greater or less deviation of the vapour from the gaseous condition consists in a more or less marked influence which the interaction of the isolated molecules exerts on this motion, it must consequently be admitted that the unit weight of vapour, when occupying an invariable space, may, for a certain inertia of the molecular motion, display a considerable degree of the maintenance of the molecular interaction, which decreases as the motion becomes more active, entirely disappears, and then, with a greater intensity of movement, reappears and increases in energy the higher the molecular motion is raised.

This conception is difficult, it cannot be denied; but the observations compel us thereto; nothing else can be deduced from the observations, even under the assumption of the widest possible errors in them. Moreover this conception appears to me to be not at all inconsistent with the mechanical theory of heat. For since the influence which the interaction of the molecules exerts on their movement is measured by the quotient of the time during which a molecule taken at random is found within the sphere of action of other molecules, and of the time during which it moves free therefrom, and since this quotient is a function, first, of the time elapsed during a single movement of two molecules within their sphere of mutual action, and, secondly, of the repetition of such meetings, therefore it is probable, considering the utter uncertainty in which we find ourselves concerning the details of this occurrence, that at the commencement of the above-described process the first moment especially, and at the end the second moment come into account, while between them there lies a condition when both moments are of imperceptible action.

§ 9.

Very similar results are obtained from the consideration of P_1 at different temperatures, *i. e.* of the different tensions under which the vapour at each temperature first enters into the gaseous condition. Representing graphically the connexion of the tensions p_1 and P_1 with the temperature (fig. 3), the curve p_1 becomes

the well-known tension-curve, which constantly withdraws itself from the axis of abscissæ as the temperature increases. At the abscissa $a+9{\cdot}5$ the curve P_1 coincides with the curve p_1, but afterwards at higher temperatures approaches nearer to the axis. And here the vapours of chloroform and of bisulphide of carbon show that the curve P_1 may have a maximum. Now, since we cannot assume that, beyond the maximum, P_1 constantly decreases with an increasing temperature, therefore the curve P_1 after the maximum must have a minimum, in order that it may withdraw itself more and more from the axis of abscissæ, as is approximately shown in fig. 3.

Consequently we may draw a parallel to the axis of abscissæ from a point on the curve p_1 which shall cut the curve P_1 three times; *i. e.* the same tension of the vapour may correspond to the superheated condition for lower temperatures and to the gaseous for higher temperatures, then the vapour may enter again into the superheated, and finally into the gaseous state. This conclusion is connected with that drawn previously from the course of V_1, since the product P_1V_1 must increase proportionately to the absolute temperature (the abscissa in the diagram).

§ 10.

From what has been said in the last two paragraphs, a surprising conclusion is arrived at concerning the behaviour of the coefficients of dilatation of vapours of constant volume and under a constant pressure.

Since the superheated condition shows a smaller product pv than there would be in the corresponding gaseous condition, it follows that whenever a constant volume v is taken, the pressure p must be smaller for superheated vapour than it is, under otherwise similar circumstances, for an ideal gas. Hence it follows that when the vapour under a constant volume and with a gradually increasing temperature passes gradually from the superheated condition into the gaseous condition, it must exhibit a greater coefficient of dilatation for a constant volume than that of an ideal gas would be; and, conversely, in a gradual progress from the gaseous to the superheated condition with an increasing temperature the coefficient of dilatation of the vapour under a constant volume must be smaller than that of an ideal gas. All this holds good when the volume v is interchanged with the pressure p for the coefficient of dilatation under a constant pressure.

From the particulars of the curve V_1, as represented in fig. 2, it follows that the coefficient of dilatation can exhibit the behaviour of the vapour in its dependence on the temperature when under a constant volume, as is given in fig. 4, where the abscissæ x represent the temperature, the ordinates y the coefficient of dila-

tation, and the parallel to the axis of abscissæ MN the coefficient of dilatation of an ideal gas. The curve, fig. 4, drops from a value which is larger than that for an ideal gas, down to this value, and in its further course arrives at still smaller values.

In the same way, from the particulars of the curve P_1 (fig. 3), it follows that the coefficient of dilatation of a vapour under a constant pressure may depend on the temperature in the manner shown in fig. 5, which is arranged after the fashion of fig. 4. The curve (fig. 5) crosses the line which represents the coefficient of dilatation of an ideal gas, so that, starting from greater values, it meets this line, further on it arrives at a minimum lying under it, then rises to a maximum which lies over it, and finally, from a certain high temperature, runs into and along with it.

Since the last part of the curve in fig. 4 lies always below the line of the coefficient of gas, while the last part of the curve in fig. 5 coincides with this line, consequently, for such vapours as those of chloroform and sulphuret of carbon, the coefficient of dilatation at and from a certain high temperature is much smaller for a constant volume than for a constant pressure, a property which reminds us of Regnault's experiments on the so-called permanent gases.

On looking back, I find that some other observations besides those here communicated on the coefficient of dilatation under a constant pressure appear to point to such a behaviour of this coefficient as is represented in fig. 5. That the curve has most probably the maximum lying in the neighbourhood of B (fig. 5) has been observed by Deville and Troost* in the case of vapour of hyponitric acid under a pressure of one atmosphere. Any way the want of any other explanation can no longer make it necessary to assume a dissociation of hyponitric acid. Of course in case such relations be assumed for hyponitric acid as I have found for chloroform and bisulphide of carbon, and there also the validity of the relation $\frac{PV}{p_1 v_1} = c\sqrt{a+t}$ be supposed, the constant c must have a much larger value than ·0595, since the density found at 26°·7 of the vapour when all but saturated differs considerably more from the final vapour-density than it would on the assumption of the constant $c = \cdot 0595$. I will not make any assertion respecting this case; I wish only to suggest the possibility of such an explanation of the results of Deville and Troost.

On the other hand, we may perhaps deduce the existence of the minimum of the curve in fig. 5 which lies at A, from Hirn's researches quoted in § 1. Hirn gives, amongst other examples, the specific volumes of superheated aqueous vapour under a

* *Comptes Rendus*, vol. lxiv. p. 237.

pressure of one atmosphere for several temperatures. Although, for the right determination of the coefficient of dilatation under a constant pressure acccording to the formula $\alpha = \frac{1}{v\frac{dt}{dv} - 1}$ the correct knowledge of the relation existing between t and v is indispensable, we may yet calculate approximately, by help of the not very large differences of the volumes here treated of, the mean coefficient of dilatation between each pair of temperatures by means of the formula $\alpha = \frac{v_0 - v}{vt_0 - v_0 t}$. Thereby we obtain from Hirn's statements the following values:—

Temperature, Celsius.	Mean coefficients of dilatation.
100° } 118·5 }	0·004181
118·5 } 141 }	0·004212
141 } 162 }	0·002902
162 } 200 }	0·003059
200 } 246·5 }	0·003838

These numbers naturally give only a very rough picture of the relations which hold; but perhaps their course is sufficiently determinate to point to some such minimum of the coefficient of dilatation as my own observations have given for chloroform and bisulphide of carbon, of course under a less pressure.

§ 11.

From the considerations set forth in the last paragraph, we may see that such a form of the equation of condition as Zeuner first gave, in the *Zeitschrift des Vereins deutscher Ingenieure*, 1867, p. 49, for superheated aqueous vapour, $pv = B(a+t) - Cp^{\frac{k-1}{k}}$, where B, C, and k are constant, cannot be employed with certainty for the vapours of chloroform and of bisulphide of carbon consistently with the observations here communicated. Indeed according to this equation a course of the curve P_1 similar to that described would not be possible. Suppose in the equation the pressure p constant, then it follows from $B(a+t) - pv =$ const. that pv will correspond more closely with $B(a+t)$, i. e. will approach more nearly to a gaseous condition, the larger that $(a+t)$ is. Therefore for a constant pressure with an increasing temperature the vapour must, in conformity with this equation, be continually approximating to it.

Zeuner deduces the equation on the grounds of two assumptions: (1) that the specific heat of aqueous vapour is constant

under a constant pressure c_p; and (2) that for aqueous vapour, as well as for the gases, the relation $\frac{a+t}{a+t_0}=\left(\frac{p}{po}\right)^{\frac{k-1}{k}}$ holds at two points of the same unicursal curve, where the constant k has, of course, a different value from what it has in the case of gases. Zeuner holds, moreover, even a slight variation of the quantities c_p and k to be not unlikely, wherefore the above equation would give the true relations approximately only.

We may remark that the assumption of a constant c for vapours generally is not supported by Regnault's experiments* in a conclusive manner; and for bisulphide of carbon it appears even more decisively that a variation of c_p must result from Regnault's figures.

I am engaged in a further prosecution of the observations here communicated, and will report them hereafter.

Bonn, February 2, 1869.

XXXV. *On the Nebular Hypothesis.*

By J. S. Aldis, *M.A., late Scholar of Trinity College, Cambridge*†.

The test of theory is the deduction of numerical results. It is not very easy to obtain such results from the nebular hypothesis. Still the principle of the conservation of areas affords a few results hitherto, we believe, unnoticed. They are not, however, altogether free from objection.

Every planet when detached from the central body, whether as a ring or (as appears to us more probable) as the stalk end of the pear-shaped central mass, must have rotated on its axis in its periodic time round the central body. Assuming, then, that the portions when detached did not differ much from a sphere in shape, we calculate the densities of the different planets when first detached. They are as follows, that of the earth being unity:—

Mercury	·00113
Venus	·000241
Earth and Moon . .	·0000128
Mars	·0000411
Jupiter	·00000023
Saturn	·0000000285

These results are strikingly in accordance with theory. Mercury, Venus, and Mars have their densities nearly inversely as the cubes of their distances, the others not. Whatever the law

* *Mém. de l'Acad.* vol. xxvi. p. 163.

† Communicated by the Author.

of density in the central body, the exterior part would vary in density according to such a law, as that body contracted.

Those that do not obey this law confirm the nebular hypothesis quite as well. They all have satellites, and have them because the detached body was not spherical but elongated, the stalk end of the pear-shaped mass being unusually long. The impossibility of homogeneity necessarily would develope a pear-shaped mass in the cooling down of the central body rather than the ellipsoidal or spheroidal. The density of the earth should be about seven or eight times what the above Table indicates, for it to vary between the inverse square and inverse cube (the law is nearly the inverse cube, but not quite); and to have that increased density with the same amount of angular momentum in the detached body, the latter should be some three or four times as long as broad, and hence in contracting, as it would in length be about double the distance of the earth from the moon, it would naturally separate into two bodies. The moon's original day apparently was nearly half as long again as that of the earth.

The densities of Jupiter and Saturn are far less than the law would give, and due to the same cause, since they abound with satellites, though the great gap between Mars and Jupiter strongly suggests those nebulæ where a central mass is surrounded by a ring, on the outskirts of which hang smaller nebulæ.

There is connected with this hypothesis a point in the structure of the earth deserving attention. It has been remarked that there is a tendency in mountain-chains to run north and south, and to present steep slopes to the west, gentle declivities to the east. This may arise from the contraction of the earth. If a portion of unsupported crust sink towards the centre, it will subside on to that which is moving east less rapidly than itself, and in consequence will, so to speak, fall over towards the east, the surface forming a gradual slope to the east, and the fractured western edge a precipitous descent to the west.

In the moon, too, we see proofs of the contraction continued long after the stage in which we now find the earth. The spheroid of the moon has contracted since it assumed that shape, and, contracting less in the longer diameter, is now more spheroidal than it should be according to theory, whilst the thickened crust, no longer crushed down on the interior, has left cavities in which the moon's ocean and atmosphere are entombed for ever.

Manchester Free Grammar School,
September 16, 1869.

XXXVI. *Thermal Researches on the Battery.* *By* M. P. A. Favre*.

I HAVE formerly insisted upon the utility of considering, in the investigation of voltaic currents, the absolute quantity of heat put in play in the whole circuit and in each of its parts. The investigations contained in this paper have principally for their object to ascertain the origin of the heat which is not found in the circuit, and which is confined to the couples. As in this abstract I cannot produce the numerous Tables referring to the various series of experiments, I shall restrict myself to indicating the tendency of the results and the conclusions which seem to follow from them.

I. I repeated Pouillet's experiments on the intensity of the current according as we work with a single couple or with a battery of any number of couples—the electromotive force and the internal resistance of each couple being equal, and the external resistance R either equal to zero or varied by the introduction of different lengths of wire. Working under these conditions, I restricted myself to investigating the distribution of heat corresponding to the resistances R and r of the circuit.

I worked successively with one, two, three, four, and five couples†, and found that for the same amount of chemical action and the same finite value of R, the quantity of heat due to the internal resistance of the battery was greater than the quantity due to that of the couple. Thus the calorific effects in both cases are in the direction which Pouillet had remarked for the intensities.

II. I repeated the same experiments a great number of times in succession, and without renewing the liquid, until at least half of the sulphuric acid was changed into sulphate of zinc. It was difficult to exceed this limit; for when I made several couples work simultaneously, R being equal to 0, the platinum of one or more couples became covered with so large a quantity of zinc, that it could not dissolve with sufficient rapidity, rendering impossible any calorimetric determination.

In each series of experiments R was made alternately $=0$ and $=250$, 500, and up to 7000 millims. of my normal platinum wire, so that I could calculate the internal resistance of the couple or the battery in each of the successive experiments.

I give here the numbers furnished by the first and the last operation of one of the series of experiments made by means of

* Translated from the *Comptes Rendus*, November 23, 1868.

† The liquid was renewed each time.

a battery of five elements. The acid was pure* in the first operation, while, in the last, half of it had been replaced by sulphate of zinc†.

	Value of r. millims.	Heat confined to the battery. units.	Heat expended in 7000 millims. of wire. units.
(1)	70	1994	17840
(2)	106	9282	10552

whence

	Total heat of the circuit $R+r$. units.	Heat confined to the battery. units.
(1)	18018	1816
(2)	10712	9122

What is the origin of this quantity of heat which thus remains confined within the battery‡?

It seems to me that it can only be explained on the assumption that the following actions come into play either together or separately:—(1) the condensation of hydrogen upon the platinum, which becomes an obstacle to the transmission of the current; (2) the local action due to the passage of the hydrogen from the *nascent* to the *ordinary* state; (3) the action, also local, due to the sulphatation of the zinc deposited on the platinum plates—a deposit arising from the electrolysis of sulphate of zinc, as this salt continually increases in the liquid in which the couples are immersed.

I will first remark that if the hydrogen offers a passive resistance to the passage of the current, this resistance is included in the internal resistance of the battery, the thermal constituent of which has already been calculated. Moreover I estimate that no considerable fraction of the quantity of heat indicated by the calorimeter in which is the battery (a quantity which increases with the number of anterior operations) can be attributed to the influence of the condensed hydrogen.

III. I have confirmed a fact already stated by several physicists, that the quantity of hydrogen condensed on the surface of the platinum is very small, and does not go on increasing indefinitely. Working with two of Smee's elements joined to-

* The sulphuric acid used, of a given degree of dilution, liberated 19,834 thermal units in acting upon zinc.

† I may mention that in my couples the passive resistance which the sulphate of zinc presents to the current is sensibly equal to that presented by sulphuric acid.

‡ In my previous experiments I had found this quantity equal sometimes to 4000, sometimes to about 6000 units; the variation is much greater in the present experiments (from 1800 to 2000), but under well-defined conditions.

gether, I measured the gases which they separately disengaged. One of these elements, having served for various operations before being used for the present experiment, was covered with all the hydrogen it could condense, while the other, working for the first time, had not been able to condense hydrogen on its surface.

I then took a new couple, the platinum of which had been treated with boiling nitric acid, then heated to redness, and immersed in a considerable mass (about 2 litres) of my normal acid*. The intensity of the current did not appreciably vary in the numerous successive experiments, and the quantity of heat indicated by the calorimeter containing a rheostat was virtually the same. Hence the hydrogen condensed on the surface of the platinum does not exercise any appreciable influence on the phenomenon in question, and the variations observed should be attributed to the differences of chemical composition which the liquid of the couple experiences under ordinary circumstances.

Lastly, it is sufficient to renew the liquid of the couples of Smee's battery which have worked for some time, in order to recover the original intensity and the corresponding thermal result.

The influence of the other causes above mentioned has still to be investigated.

I will first observe that in the first of the experiments II. the local phenomenon of the solution of the zinc deposited on the platinum in the acid can only play a very small part in the 1816 thermal units indicated by the calorimeter in which is the battery. In fact, at the beginning of the experiment there is no sulphate of zinc in the liquid; and the absolute quantity at the end is very small, while the sulphuric acid which remains free is in a relatively large proportion (only about $\frac{1}{50}$ of the sulphuric acid has been changed into sulphate of zinc). Hence I have necessarily been led to attribute the heat which remains in the couples whenever the acid liquid is renewed, almost exclusively to the local phenomenon of the change of condition of the hydrogen. May we consider the number adduced of 1816 units as representing even approximately the effect due to the change of state of the hydrogen? I think not; for the quantity of heat corresponding to the chemical action, which is not met with in the circuit $R+r$ and which is confined to the couples, is greater (other things being equal) the shorter the time in which the electrolysis of sulphuric acid is effected.

IV. The following numbers justify this assertion; they correspond to experiments in which the liquid of the battery was renewed each time, and in which the length of the platinum wire in the external part of the circuit was successively reduced:—

* The quantity of acid ordinarily employed is 90 cubic centims.

Value of R.	Heat confined within the battery.	Heat corresponding to $R+r$.
millims.	units.	units.
7000	1816	18018
4000	2349	17485
1000	3373	18461
500	4777	15057
250	5410	14424

V. Resuming my determinations of the electrolysis of sulphate of copper and sulphate of hydrogen*, and varying the conditions of the experiments, I obtained a number higher than those I have given, and which must be nearer the real number representing the change of condition of hydrogen. This number, which is about 6000 thermal units, is but little different from that given in my previous memoir.

In the present series of researches I have taken the precaution to collect and analyze the gases disengaged in the voltameter, and to allow for the formation of oxygenated water and for the water which is reformed.

When, instead of working with pure and renewed acid, the proportion of sulphate of zinc is allowed to increase, the influence due to the electrolysis of the salt is soon evident. In consequence of it a deposit of zinc is formed on the surface of the platinum. In dissolving, this produces a quantity of heat which is not transmissible to the circuit—a fact which explains the number 9122 which expresses the quantity of heat which is not found in the circuit $R+r$, and which the causes previously investigated would not have produced.

VI. In fact, when we examine what takes place in a battery of several Smee's elements, we see, when by successive operations the liquid has become charged with sulphate of zinc, that one or several of the couples scarcely disengage any gas-bubbles; then when the circuit is opened, the couples disengage more and more rapidly the complement of gas, forming for each element a total equal to that which had been disengaged by each couple working regularly to the moment of opening the circuit.

The same phenomenon is produced with a single couple, and becomes markedly apparent; for the disengagement of gas is seen to continue for a certain time after the circuit is opened, and then suddenly to stop†.

The quantity of sulphate of zinc thus decomposed, and the acid of which being liberated attacks the zinc of the couple, always corresponds to an equivalent quantity of sulphuric acid which does not come into play in the reaction; so that for the same intensity there is always the same amount of zinc attacked

* *Comptes Rendus*, vol. lxvi. Feb. 10, 1868.

† I may remark that, the circuit being open, the zinc may remain immersed for a whole week without any gas being liberated.

to the advantage of the current; but as the metalloid radical SO^4 which attacks the zinc is not solely taken from the sulphuric acid but comes partially from the dissolved sulphate of zinc, it follows that the electromotive force, and therefore the power of the battery, diminishes proportionally to the quantity of heat necessary for the electrolysis of this latter salt.

To the electrolysis of zinc, therefore, we must principally attribute the want of constancy in the intensity of the current furnished by a Smee's couple*.

VII. Substituting amalgamated cadmium for zinc in the formation of the couples, I observed perfectly similar results.

VIII. Finally I introduced into the part of the circuit exterior to the calorimeter which contained the pile a rheostat, in some cases at the ordinary temperature, and in some heated to bright redness. In the latter case the resistance was almost doubled, and the quantity of heat furnished by the battery was that which would have been taken from it by a rheostat of double the length and kept at the ordinary temperature. I shall soon revert to this subject.

XXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 162.]

May 27, 1869.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read:—

"On the Radiation of Heat from the Moon." By the Earl of Rosse, F.R.S.

The following experiments on Lunar Radiant Heat were undertaken with the view of ascertaining whether with more powerful and more suitable means than those previously employed by others, with little or no success, it would be possible to detect and estimate the amount of heat which reaches the earth's surface from the moon.

Professor Piazzi Smyth had conducted a series of experiments on the Peak of Teneriffe with a thermopile, but apparently without any means of concentrating the moon's heat beyond the ordinary polished metal cone.

Melloni had employed a glass lens of considerable diameter (I believe about three feet); but as glass absorbs rays of low refrangibility, it was not so well adapted to concentrate heat as a metallic mirror.

In the following experiments the point sought to be determined was, in what proportions the moon's heat consists of:—

(1) That coming from the interior of the moon, which will not vary with the phase:—

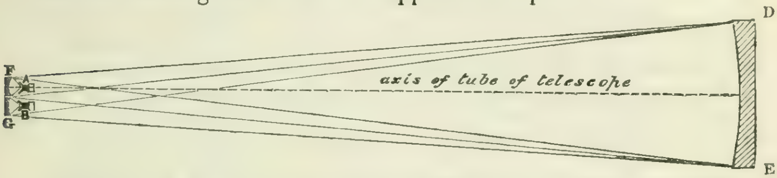
* I preferred the use of Smee's battery in my researches, because I was not concerned with the constancy of the current, and it is both rapid and easy to work with.

(2) That which falls from the sun on the moon's surface, and is at once reflected regularly and irregularly.

(3) That which, falling from the sun on the moon's surface, is absorbed, raises the temperature of the moon's surface, and is afterwards radiated as heat of low refrangibility.

The apparatus consisted of a thermopile of four elements, the faces half an inch square, on which all the moon's heat which falls on the large speculum of the 3-foot telescope is concentrated, by means of a concave mirror of $3\frac{1}{2}$ inches aperture, 2·8 inches focal length.

As it was found difficult to compensate the effects of unequal radiation on the anterior face of the pile, by exposing the posterior face also of the *same* pile to radiation from the sky, during the later experiments (beginning with March 23rd) two piles were used, and the following was the form of apparatus adopted.



D E is the large mirror of the telescope; F G the two small concave mirrors of $3\frac{1}{2}$ inches aperture, and 2·8 inches focal length, fixed in the plane of the image formed by the large mirror D E. The two thermopiles are placed respectively in the foci of F and G, their anterior faces shielded from wind and other disturbing causes by polished brass cones, and their posterior faces kept at a nearly uniform temperature by means of brass caps filled with water. The thermopiles and accompanying mirrors are supported by a bar screwed temporarily on the mouth of the tube. Two wires are connected with the two poles of each pile; and the ends of the wires are connected, two and two, close to the galvanometer, in such a manner that a given amount of heat on the anterior face of one pile will produce a deviation equal in amount, and opposite in direction, to that produced by an equal amount of heat on the anterior face of the other pile. Thomson's Reflecting Galvanometer was the one used.

This apparatus has not yet had a fair trial, as I was unable to obtain from Messrs. Elliot a pile ready made of similar dimensions to that which I already possessed. That which they sent had only one-fourth the required area of face.

The following Table (p. 316) is a summary of the results.

In column 3 is given the mean of the deviations of all the single differences from the mean difference of all the readings taken with the moon on and with the moon off the apparatus.

In column 4 the arithmetic mean of all the observed deviations.

In column 5 the calculated deviation for each night at midnight, on the assumption that the deviation corresponding to full moon $=100$, and that the moon is a smooth sphere.

Reference number.	Date of observation.	Mean error.	Mean deviation.	Deviation (calculated).	Observed deviation reduced to full moon.	180° − moon's distance from the sun.	Mean altitude of moon.	Number of readings.	
	1868.								
I.	Dec. 30.	...	103·7	94·1	110	19°			
II.	„ 31.	...	85·1	85·8	99·2	33			
	1869.								
III.	Jan. 1.	...	67·5	73·1	92·1	47			
IV.	„ 21.	...	34	41·9	81·1	79	...	...	Occasional clouds.
V.	„ 26.	...	83	96·7	85·8	15	56°	...	White frost. Mirrors became dewed; but the readings taken after this took place have been rejected.
VI.	Mar. 23.	34	57	67·7	84·2	57	...	40	Occasional clouds.
VII.	„ 27.	49	115	99·6	115	5	35	15	Occasional clouds, strong gusts of wind.
VIII.	„ 28.	35	113	96·1	117	16	30	49	No note of cloud, very little breeze, generally calm.
IX.	„ 31.	...	17	62·8	27·7	58	18	31	Moon low, sky covered with hazy clouds, through which the moon was seen with much diminished brilliancy.
X.	April 14.	...	......	8·3	......	123	...	4	Very clear and calm, but moon low; no perceptible impulse imparted to the needle.
XI.	„ 17.	27	13·1	16·6	79	110	27	65	Wind blowing strong into the mouth of the tube nearly the whole time.
XII.	„ 19.	43	35·5	36·3	96	85	25	14	No note of cloud till just at the end of these observations.
XIII.	„ 20.	85	33	48·8	68	72	35	51	A very little wind; occasional clouds.
XIV.	„ 22.	38	12·1	75·5	......	45	...	15	Halo with hazy clouds; moon seen through them with much-diminished brilliancy.
XV.	„ 24.	28	84	95·3	88·2	18	30	29	Frequent passing clouds during the latter part of these observations.
XVI.	„ 25.	45	88·4	99·4	88·8	6	25	66	No cloud visible, but haziness suspected, as it existed both at sunset and at sunrise.
1	2	3	4	5	6	7	8	9	

We have then Q (quantity of heat coming from the moon's surface)

$$=C\int_{\epsilon-\frac{\pi}{2}}^{\frac{\pi}{2}}\cos\theta\,.\,\cos(\epsilon-\theta)\,d\theta$$

$$=\frac{C}{2}\,\{\overline{\pi-\epsilon}\,.\,\cos\epsilon+\sin\epsilon\}^*,$$

* This formula is based on the assumption that the heat coming to the earth

where $\epsilon=\pi-$apparent distance between the centres of the sun and moon.

$$\text{When } \epsilon=0 \text{ (full moon), } Q=\frac{C}{2}\cdot\pi,$$

$$\text{when } \epsilon=\frac{\pi}{2} \text{ (half moon), } Q=\frac{C}{2},$$

$$\text{when } \epsilon=\pi \text{ (new moon), } Q=0;$$

$\therefore$ if full moon $=100$, Q in general

$$=100\left(1-\frac{\epsilon}{\pi}\cos\epsilon+\sin\epsilon\right). \quad . \quad . \quad . \quad . \quad (a)$$

In column 6 we have the deviation for full moon calculated from the observed mean deviation for each night.

In column 7 the supplement of the apparent distance between the centres of the sun and moon.

In column 8 the approximate mean altitude of the moon.

In column 9 the number of times the telescope was put on or off the moon during the observations included in the mean result.

In all these observations the deviations which have been measured are those due to the difference between the radiation from a circle of sky containing the moon's disk, and that from a similar circle of sky close to it not containing the moon's disk.

The annexed diagram will show approximately the rate at which the moon's light increases and diminishes with its phases as deduced from formula (*a*); and the ringed dots with the accompanying Roman figures (for reference) give the quantity of the moon's heat as determined by observation on different nights.

Although there is considerable discordance between some of the observed and calculated quantities of heat, the results suggest to us that the law of variation of the moon's heat will probably be found not to differ much from that of the moon's light. It therefore follows that not more than a small part of the moon's heat can come from the first of the three sources already mentioned.

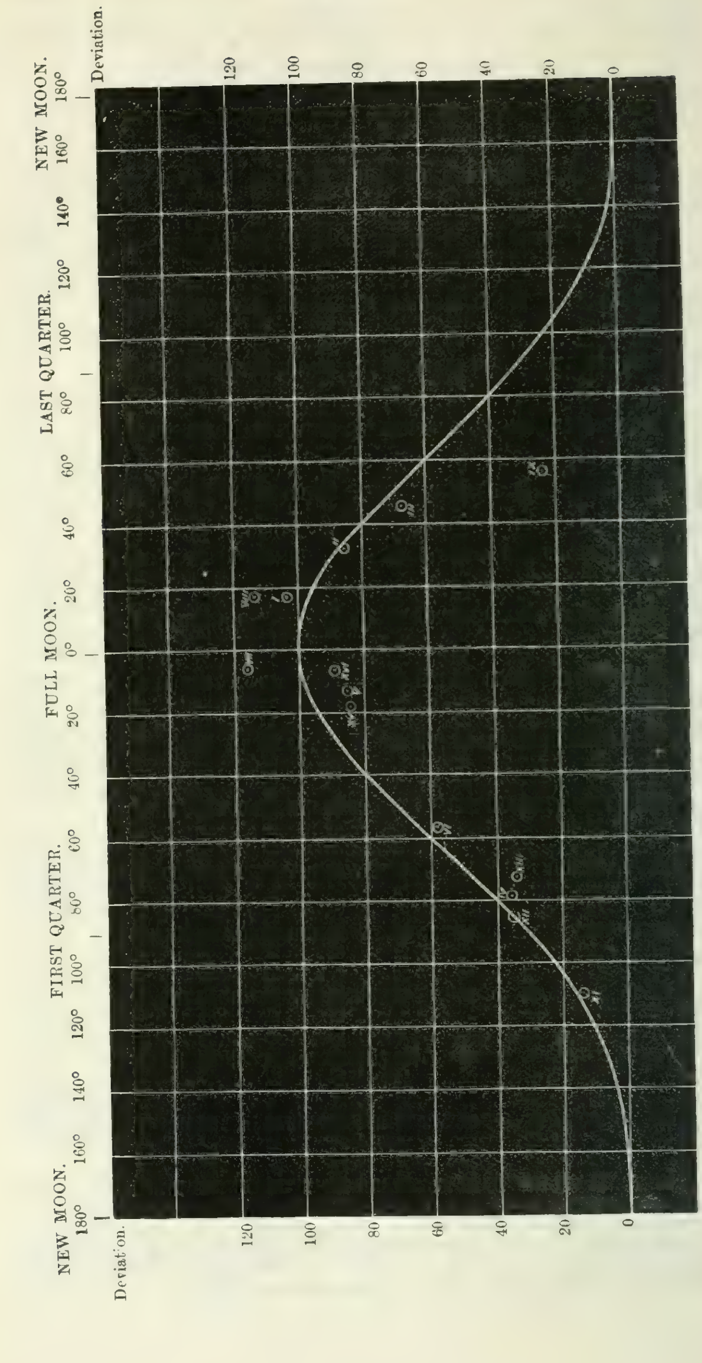
With the view of ascertaining what proportion of the sun's heat does not leave the moon's surface until after it has been absorbed, some readings of the galvanometer were taken on four different nights near the time of full moon, with a disk of thin plate glass in front of the face of each pile; and the deviation was about six or eight divisions.

As the glass screens were examined with care for dew after removal on each night, and none was perceived except on one occasion, the probable percentage of the *moon's* heat which passes through plate glass is 8, or rather less.

Few experiments appear to have been made on the absorptive power of glass for the *sun's* rays; but, from the best data that I have been able to obtain, I find that probably about 80 per cent. pass through glass.

The greater part of the moon's heat which reaches the earth appears, therefore, to have been first absorbed by the lunar surface.

from an element (δS) of the moon's surface $=K.\delta S.\cos\theta.\cos\phi$, θ and ϕ being respectively the inclinations of the lines to the sun and to the earth from the normal to that point of the moon's surface, and K a constant.


NEW MOON.
LAST QUARTER.
FULL MOON.
FIRST QUARTER.
NEW MOON.
Deviation.
Deviation.
180°
160°
140°
120°
100°
80°
60°
40°
20°
0°
20°
40°
60°
80°
100°
120°
140°
160°
180°
120
100
80
60
40
20
0
120
100
80
60
40
20
0

It now appeared desirable to verify this result, as far as possible, by determining by direct experiment the proportion which exists between the heat which reaches the earth from the sun and from the moon.

If we start with the assumption that the sun's heat is composed of two portions,

the luminous rays, whose amount $=\mathrm{L}$,

and the non-luminous, ,, ,, $=\mathrm{O}$,

also that the moon's light consists of two corresponding portions, L', O', the luminous not being absorbed, and the non-luminous being entirely absorbed in their passage through glass, then

$$\left.\begin{aligned}\frac{\mathrm{L}}{\mathrm{L}+\mathrm{O}}&=\cdot 8,\\ \frac{\mathrm{L}'}{\mathrm{L}'+\mathrm{O}'}&=\cdot 08;\end{aligned}\right\}\quad \therefore\ \frac{\mathrm{L}}{\mathrm{L}'}\times\frac{\mathrm{L}'+\mathrm{O}'}{\mathrm{L}+\mathrm{O}}=10.$$

Substituting for $\frac{\mathrm{L}}{\mathrm{L}'}$ its generally received value (800,000), we have

$$\frac{\mathrm{L}'+\mathrm{O}'}{\mathrm{L}+\mathrm{O}}=\frac{1}{80,000}. \quad \ldots\ldots\ldots\ldots \quad (b)$$

Owing to the extremely uncertain state of the weather, only one series of eighteen readings was obtained for the determination of the sun's heat. A beam of sunlight was thrown, by means of a plane mirror, alternately on and off a plate of polished metal with a hole ·175 inch in diameter. At a short distance behind this the pile was placed. The deviation thus found was connected with that previously found for full moon by using the deviation produced by a vessel of hot water as a term of comparison.

The relative amount of solar and lunar radiation thus found was

$$89819:1, \quad \ldots\ldots\ldots\ldots \quad (c)$$

which is quite as near that given by (b) as we could expect when we consider the roughness of the data.

As a further confirmation of the correctness of the two rough approximations to the value of the ratio existing between the sun's and the moon's radiant heat already given, the subject was investigated from a purely theoretical point of view. It was assumed

(1) That the quantity of heat leaving the moon at any instant may without much error be considered the same as that falling on it at that instant.

(2) That the absorptive power of our atmosphere is the same for lunar and solar heat.

(3) That, as was already assumed in obtaining formula (a), the moon is a *smooth* sphere not capable of reflecting light *regularly*. Then the heat which leaves the moon in all directions = quantity which falls on the moon $=\frac{1}{13\cdot 55}$ of the quantity which falls on the earth from the sun

$$=\mathrm{K}\,.\int_0^{\pi}\{(\pi-\epsilon)\,.\cos\epsilon+\sin\epsilon\}\sin\epsilon\,.\,d\epsilon=\frac{\mathrm{K}}{4}\,3\pi.$$

The part which falls on the earth

$$=\mathrm{K}\,.\int_0^{\frac{1}{59\cdot 964}}\{(\pi-\epsilon)\cos\epsilon+\sin\epsilon\}\sin\epsilon\,.\,d\epsilon$$

$$=\frac{\mathrm{K}}{4}\times\left\{-\pi\,.\,\text{versin}\,(1^\circ\ 55')+\frac{2+\cos\,(1^\circ\ 55')}{59\cdot 964}-\tfrac{8}{3}\sin(1^\circ 55')\right\}$$

$$=\frac{\mathrm{K}}{4}\,.\,\mathrm{E}\ \text{suppose};$$

therefore (if we may be allowed the expression)

$$\frac{\text{sun-heat}}{\text{moon-heat}}=\frac{13\cdot 55\times 3\pi}{\mathrm{E}}=\frac{79{,}000}{1}\ \text{(quam proximè)}.\quad .\quad .\quad .\quad (d)$$

In the above, the proportion between the areas of surface presented by the moon and earth to the sun is taken $=13\cdot 55$, and the angle subtended by the earth at the moon $=1^\circ\ 55'$.

The value of the readings of the galvanometer was determined by comparison with those obtained by using a vessel of hot water coated with shellac and lampblack varnish as a source of heat. The vessel was of tin, circular, and subtended the same angle at the small concave reflectors as the large mirror of the telescope. It was thus found that (the radiating power of the moon being supposed equal to that of the lampblack surface and the earth's atmosphere not to influence the result) a deviation of 90 for full moon appears to indicate an elevation of temperature through 500° Fahr.* In deducing this result allowance has been made for the *imperfect* absorption of the sun's rays by the lunar surface.

In the present imperfect state of these observations it would be premature to discuss them at greater length; but as some months must elapse before any more complete series can be obtained, and the present results are sufficient to show conclusively that the moon's heat is capable of being detected with certainty by the thermopile, I have thought it best to send this account to the Royal Society; and I shall be most happy to receive suggestions as to improvements in the method of working, and as to the direction in which it may be most desirable to carry on future experiments.

GEOLOGICAL SOCIETY.

[Continued from p. 243.]

February 10th, 1869.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following papers were read:—

* This may seem a very large rise of temperature; but it is quite in accordance with the views of Sir John Herschel on the subject (Outlines of Astronomy, section 432 and preceding sections), where he says that, in consequence of the long period of rotation of the moon on its axis, and still more the absence of an atmosphere, "The climate of the moon must be most extraordinary, the alternation being that of unmitigated and burning sunshine, fiercer than that of an equatorial noon, and the keenest severity of frost, far exceeding that of our polar winters, for an equal time." And again, ". . . . the surface of the full moon exposed to us must necessarily be very much heated, possibly to a degree much exceeding that of boiling water."

1. "On the Evidence of a ridge of Lower Carboniferous Rocks crossing the Plain of Cheshire beneath the Trias, and forming the boundary between the Permian Rocks of the Lancashire type on the North and those of the Salopian type on the South." By Edward Hull, Esq., M.A., F.R.S., F.G.S.

In this paper the author proposed to account for the dissimilarity of mineral and stratigraphical characters of the Permian formation of Lancashire and the North of England as compared with that of the Midland Counties and Shropshire, on the ground that they had originally been deposited in separate basins, divided off from each other by a ridge of Lower Carboniferous rocks, stretching from west to east, under the central plain of Cheshire.

The author showed that there was evidence of such a ridge on the east side of the plain of Cheshire, by the uprise of the Lower Carboniferous rocks to the north of Congleton Edge, in the valley of the River Dane, and that the date of this uprise and the denudation of the Upper Carboniferous beds along the axis of elevation was clearly determined to be antecedent to the Permian period by the outlier of Permian rocks at Rushton Spencer.

On the west side of the plain there was evidence of a similar axis of upheaval to the south of the Flintshire Coal-field near Hope, where the Lower Carboniferous rocks (Yoredale and Millstone beds) are brought up to the surface at the margin of the New Red Sandstone.

Mr. Hull regarded the uprise on each side of the plain as referable to the same Prepermian age, and as belonging to the East and West system of flexures into which the Carboniferous rocks were thrown at the close of the Carboniferous period over the north of England. Such an axis had its antetype in the concealed ridge which once occupied the valley of the Severn, and divided the Devonian rocks of Devonshire from those of South Wales; and the author suggested that a similar ridge, now concealed beneath the Triassic formation of Cheshire, offered the only satisfactory explanation of the dissimilarity in the two types of Permian beds—that of Lancashire, and that of Shropshire and the Midland Counties.

2. "On the Red Chalk of Hunstanton." By the Rev. T. Wiltshire, M.A., F.L.S., F.G.S.

The author described the section exposed in Hunstanton Cliff as showing:—1. White Chalk with fragments of *Inocerami*. 2. White Chalk with *Siphonia paradoxica*, having its base undulated and the cavities filled up with a thin bright red, argillaceous layer, resting upon (3) the Red Chalk, which is divisible into three sections:—*a*, hard, containing *Avicula gryphæoides* and *Siphonia paradoxica*, and with fragments of *Inocerami* at its base; *b*, hard, rich in *Belemnites*; *c*, incoherent at its base, rich in *Terebratulæ*. 4. Carstone, a yellow, coarse, sandy deposit, resting on a bed of clay, containing no fossils in its upper part, but with a band of nodules containing *Ammonites Deshayesii* and other species about 30 feet down, together with ironstone nodules like those of the Lower Greensand of the Isle of Wight, and bearing impressions of fossils which correlate the lower part of the Carstone with the base of the English Lower Greensand.

The author gave a list of these fossils, and also of those of the Red Chalk, the latter amounting to sixty-one, and presenting a mixture of forms belonging to the Lower Chalk, Upper Greensand, and Gault. On comparison with the Gault section at Folkestone, the author considered it evident that the Red Chalk of Hunstanton was equivalent to the upper part of that formation. He mentioned that ten miles south of Hunstanton, in artificial sections, blue gault has been found resting upon the Carstone, whilst rather nearer to Hunstanton the same place was occupied by a red clay, connecting the two dissimilar deposits, which, however, were shown by analysis to contain nearly equal quantities of iron. If the Upper Greensand be represented in the Hunstanton section, the author considered that its place must be in the band numbered 2, containing *Siphonia paradoxica* and *Avicula gryphæoides.*

XXXVIII. *Intelligence and Miscellaneous Articles.*

ON THE EXPANSION OF GASES.

NOTE BY M. A. CAZIN, PRESENTED BY M. LEVERRIER.

IN 1862* I gave an experimental method for making known the relation that exists between the pressure and the specific weight of a gaseous mass when it expands without losing or receiving heat. At that time I had applied this method between limits of pressure only slightly differing from one another, not having the requisite apparatus. I have now been able to work up to a pressure of 9 atmospheres; and it is the result of these new experiments that I have the honour to communicate to the Academy.

The apparatus is set up in a hall of the observatory. I owe a part of the materials of it to the Scientific Association of France, and to the generosity of M. Hugon. One of his gas-engines worked a compression-pump; and I cannot praise too highly its excellent service. Let me be allowed here to thank MM. Leverrier and Hugon for their kind assistance.

I will now sketch out the principle of my method. The gas is enclosed in two reservoirs, A and B, connected by a stopcock of large orifice (4 centims. diameter). This stopcock being closed, a pump withdraws the gas in the reservoir B and compresses it to the pressure p_1 in the reservoir A. Let us suppose that we open the stopcock and close it again at the precise moment when there is an equality of pressure on both sides of the orifice. During the flow there has been a cooling in A; then, after closing, the sides have recovered their initial temperature. The final pressure p_3 is measured; afterwards the stopcock is opened again, the equilibrium is allowed to be reestablished, and the pressure p_2 is measured. When the reservoir B is sufficiently large, this pressure does not differ appreciably from the pressure acquired by the gas at the end of the expansion. I observed this fact in pursuing a method which I have explained in a preceding communication (March 9, 1868). The gaseous mass, then, which remains in the reservoir A has passed rapidly from the pressure p_1 to the pressure p_2, and its specific gravity has passed

* *Annales de Chimie et de Physique.*

from the value ρ_1 to the value ρ_2. The quantity ρ_1 is calculated from p_1, and ρ_2 from p_3.

But it is necessary to determine whether the closing of the stopcock has been instantaneously effected at a given moment; this is the essential point of the method. For this purpose a voltaic circuit is arranged, containing an electromagnet; and the movement of the stopcock determines the closure of this circuit at the moment when the orifice is opened, and afterwards its rupture at the moment when the orifice is closed. The electromagnet moves a pencil which leaves a trace upon a sheet of paper which moves at a known rate; from the length of this trace is deduced the duration T of the opening of the stopcock. A series of experiments comprises those in which we make T vary without changing either p_1 or p_2. This series is represented by a line having for abscissæ the values of T, and for ordinates the values of $\frac{p_3-p_2}{p_1-p_2}$. The ordinates vary according to a certain law as long as T is below the duration θ which corresponds to the instant sought, and according to a different law when T is above that duration. The curve is then formed of two very different branches, the point of intersection of which is determined graphically. The abscissa and the ordinate of that point give the duration θ of the complete flow and the value of p_3 which we want.

The lower branch was virtually a right line, nearly parallel to the axis of abscissæ, which indicated a very slow heating-action on the part of the sides. Hence is deduced a correction giving the superior limit of the value that p_3 would have assumed if the sides had been impervious to heat. The feebleness of the thermal action of the sides is remarkable; we may attribute it to the formation of a gaseous sheath varnishing the sides.

First mode of observation.—p_1-p_2 is small; it is measured by means of an oil manometer, whose branches communicate respectively with the reservoir A (29 litres) and the reservoir B (520 litres), and by an open-air manometer communicating with one of the reservoirs. Similarly p_3-p_2 is measured. All necessary precautions are taken so that the gas enclosed in the manometers may not by its motion disturb the expansion. In this way I found that the quantity

$$m=\frac{\log p_1-\log p_2}{\log p_1-\log p}$$

was constant for air and carbonic acid when p_2 varied from 1 to 5 atmospheres. I did not raise the pressure higher, because the resistance of the sheet-iron reservoir B imposed this limit. Carbonic acid presented the oscillation that I described in 1862.

I concluded from this that, if one of these gases expanded in a space impervious to heat without acquiring an appreciable velocity, the law of expansion would be represented by the known formula of Laplace and Poisson,

$$p=A\rho^m,$$

A and m being two constants for the same gas; $m=1{\cdot}41$ for air, and $1{\cdot}29$ for carbonic acid.

This result is interesting as regards the mechanical theory of heat,

We know that this theory leads to this formula when we suppose the internal work due to the change undergone by the gas equal to zero. It would seem that this does not hold for carbonic acid, the internal work of which is considerable. M. Hirn has put forward a theory applicable to this case which leads to the same formula; my experiments are consequently favourable to this theory; but I ought to remark that this law represents an ideal expansion which cannot be realized, and it will be seen that real expansions comport themselves differently.

Second mode of observation.—We keep p_1 constant and vary p_2. It is thus that I have studied expansion from a pressure of 9 atmospheres to 5, 4, 3, ... atmospheres. The principal results of this investigation are given in the annexed Table.

$p_1=6576$ millims. of mercury, $\rho_1=6\cdot61302$.

The specific gravity $\rho=1$ under a pressure of 1000 millims.

p_2.	p_3.	Δt	θ.	Δy.	ρ_2.	ρ'.	ρ''.	$\rho''-\rho'$.
Air.								
		°	s.					
4219	4728·0	29·4	0·15	0·00610	4·74641	4·74861	4·82721	0·07860
2998	3685·0	50·9	0·23	0·00590	3·69553	3·70043	3·78846	0·08803
2173	2925·9	70·2	0·40	0·01080	2·93198	2·94618	3·00548	0·05930
1437	2156·5	91·0	0·54	0·01321	2·15923	2·18513	2·24883	0·06370
769	1349·7	117·4	0·70	0·02341	1·35022	1·38272	1·44338	0·06066
Carbonic acid.								
3285	3838·9	13·4	0·42	0·0045	3·93501	3·93947	4·56492	0·62545
2073	2686·8	62·3	0·64	0·0070	2·72537	2·74074	3·46253	0·72179
811	1275·1	99·3	1·12	0·0061	1·27795	1·31483	1·94386	0·62903

The specific gravities have been calculated by the help of M. Regnault's formulas for the compressibility of gases.

Δt is the decrement of the temperature, calculated according to p_1 and p_3 by means of Gay-Lussac's law.

Δy is the diminution of the ordinate for the lower branch of the curve which represents each series, corresponding to an increment of the abscissa equal to one second. It is by means of these values that the correction relative to the sides has been made.

ρ_2 is the observed specific weight without any correction.

ρ' is this weight corrected according to the thermic action of the sides.

ρ'' is this weight calculated from the formula of Laplace and Poisson with $m=1\cdot41$ for air and $1\cdot291$ for carbonic acid.

If we calculate the differences $\rho''-\rho_2$, we find quantities which, according as p_2 diminishes, vary very little for air, but which for carbonic acid first increase, then decrease. The result shows that the real specific gravity at the end of the expansion is always smaller than if the gas followed the preceding law, and that the deviation cannot be solely due to the influence of the sides; for according as

p_2 diminishes, the decrement of the temperatnre Δt increases considerably; consequently the heating by the sides ought to increase the deviation more and more if no other cause intervened. We must also remark that this deviation is greater for carbonic acid than for air, although the thermic effect of the sides is less.

I regard, then, the observed deviation as the result of two distinct causes; one is the thermic action of the sides, the other is of a different nature.

We have the effect of this latter in the last column of the Table. We see that for the two gases, $\rho''-\rho'$ begins by increasing when p_2 diminishes; this difference reaches a maximum and then decreases. Now there is a mechanical effect which varies in the same manner.

Let us consider the expansion from 9 atmospheres to 1 in two distinct cases:—

(1) Without appreciable velocities: the law is that of Laplace and Poisson.

(2) As takes place in our apparatus: the molecules situated near the orifice are animated with a certain velocity; there is in the reservoir A less gas than in the first case; according as the pressure diminishes, the velocity increases; but soon it diminishes; consequently it passes through a maximum. According to the period at which the flow is stopped, the difference of the specific gravities which exist in A in the two cases ought to vary in the same manner.

It is true that my experiments do not exactly realize the second case. Thus in the first series the expansion takes place from 9 to about 5 atmospheres, but the reservoir B is found also under a pressure of 5 atmospheres at the end of the flow; while in the last series the reservoir A is subjected to 5 atmospheres when the reservoir B is found subjected to a less pressure. However, we can conceive that this circumstance does not influence the direction of the deviation.

In fine, the formula of Laplace and Poisson can be applied to a reversible expansion; but there must be another law in the case of an irreversible expansion. The investigation of this law will be the object of a further study.

I would also remark that, the difference $\rho''-\rho'$ being greater for carbonic acid than for air, the impulse of the gas in the irreversible expansion varies in the same direction as the internal work. We meet, in short, with an effect of the *gaseous viscosity* that M. Regnault speaks of in his memoir on the velocity of sound.—*Comptes Rendus*, August 9, 1869.

ON THE EMPLOYMENT OF THE SPECTROSCOPE IN ORDER TO DISTINGUISH A FEEBLE LIGHT IN A STRONGER ONE. BY M. J. M. SEGUIN.

To the two poles of a Ruhmkorff-coil of middle size there are attached two fine platinum wires which are kept in a horizontal position, their extremities being separated by an interval of about 1 centim. The spark is produced with its usual characteristics, and we especially observe the shell of blue light which envelopes the end of the negative wire. We bring the positive wire gradually nearer

and nearer to the negative one. The latter begins to redden; at first the blue light continually grows fainter, then becomes invisible; at least we cease to distinguish the shell that it forms around the wire; and if any trace of it remains, it is only a bluish tint in the light due to the incandescence. When the wires are almost in contact, especially if the finger is presssd lightly on the hammer of the contact-breaker, the incandescence of the negative wire becomes dazzling, and then there is no more appearance of the blue light.

I was curious to know if it had really disappeared, or if it was only concealed by the brilliancy of the wire when white-hot; and I thought that the now celebrated method by which we discover the trace of the solar protuberances amongst the intenser rays of his disk might be applied here.

I made use of a vertical spectroscope by Duboscq. The slit is vertical, and can be moved from one wire to the other along the spark. The characteristics of the spectrum change according as we view the brilliant point where the spark is detached from the positive wire, or the blue shell which envelopes the extremity of the negative wire, or, finally, if that is incandescent, the red parts which lie beyond the blue shell.

We keep the slit upon the blue shell while the spark is too long to admit of the wire becoming red. The spectrum is characterized chiefly by a group of four green rays, a group of two rays placed between the green and the blue, a group of three violet rays, beyond which we can see others of less brilliancy.

As before, we gradually bring the positive wire nearer to the negative wire, which latter begins to redden. One would expect to see a continuous spectrum; and this in fact is what actually happens, if we direct the slit towards the parts of the red wire which are beyond the blue electric glow. We have then a continuous spectrum which is worth noting, because we thus learn, without requiring to light up the micrometric scale, that the violet rays given by the blue light correspond nearly to the most refrangible extremity of this continuous spectrum. Bringing back the slit to the extreme end of the negative thread, we find again the streaked spectrum of the blue light. The red in it becomes more brilliant in proportion as the thread becomes more incandescent; but the green, blue, and violet rays still continue. But when the incandescence is very intense, the green rays disappear, then the blue, and the spectrum is continuous into the violet, but at the extremity of the violet we still perceive the group of three violet rays, which become less distinct, but mark their position until the thread begins to melt. The ultra-violet rays have ceased to be visible. Thus the spectroscope permits us in this case, as well as in the observation of the solar protuberances, to ascertain the presence of a feeble glow in the midst of a light which to the direct vision is dazzling.—*Comptes Rendus*, June 7, 1869.

ON THE MEAN VELOCITY OF THE MOTION OF TRANSLATION OF THE MOLECULES IN IMPERFECT GASES. BY M. P. BLASERNA.

We are often led to inquire whence arise the deviations from

Mariotte's law that experiment reveals in the different gases. I do not think that we can accept the explanation that M. Dubrunfaut has lately offered*, an explanation which tends to ascribe these deviations to small quantities of aqueous vapour existing in even the most perfectly dried gases. When Plücker published his experiments on Geissler's tubes, I succeeded in preparing tubes of nitrogen and of carbonic acid which contained no traces whatsoever of the three brilliant rays which belong to hydrogen and aqueous vapour. To accomplish this, I made use of a good common air-pump ; I exhausted the receiver thirty or forty times, and I dried the gases by the ordinary means, except only that the electrodes were of platinum instead of aluminium, which is very often employed.

This is the method, pointed out by Rudberg, that M. Regnault and all experimenters have followed. If, nevertheless, a trace of water does remain, it seems to me impossible that it should produce the great deviations that we observe in the case of imperfect gases.

I have also proved that for air and carbonic acid the molecular state cannot be considered to result solely from mutual attractions or repulsions, whatever may be the law of these actions ; in short, a cold and expanded gas, being then heated and compressed to the same volume, ought to exhibit the same phenomena with regard to its compressibility, which is contrary to experience. And the researches of M. Amagat have lately proved the same thing for ammonia and sulphurous acid. The mechanical theory of heat leads us, as a natural consequence, to regard heat as resulting from the motions of the molecules, and to define a gas as a body whose molecules travel in all directions in space. But MM. Krönig and Clausius have shown that if we suppose these progressive motions in the gas to be rectilinear, we arrive at Mariotte's law ; and M. Clausius has even developed a formula which has enabled him to calculate the mean velocity of these motions for the better-known gases.

The deviations from Mariotte's law arise consequently from attractions which still exist in the gases, and which are nothing but a particular case of universal attraction : these attractions are more or less feeble according to the mass and the mean distances, greater or less, by which the gaseous molecules are mutually separated. This is the simplest explanation we can offer of the phenomenon ; it is the one which I believe is most generally accepted.

All this being granted, we may determine the actual velocities of the molecules in imperfect gases.

Imagine a kilogramme of gas, at temperature zero, and under an initial pressure p_0 so slight that the volume v_0 shall be very great, so that we may disregard the attractions. Increasing the pressure to p, the volume will be v'_0, and we shall have $\frac{p_0 v_0}{p v'_0} = 1 + \Delta_p$, Δ_p being the deviation from Mariotte's law under the pressure p. Raising the temperature to t, the pressure being constant, the volume v'_0 becomes v, and we have $\frac{v}{v'_0} = 1 + \alpha_p t$, α_p being the coeffi-

* *Comptes Rendus*, vol. lxviii. p. 1262.

cient of expansion under a constant pressure between 0 and t, and for the pressure p.

Putting $\frac{p_0 v_0 \alpha_0}{1+\Delta_p} = R_p$, $\frac{1}{\alpha_p} = \alpha_p$, we have

$$pv = R_p(\alpha^p + t), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

a formula which combines the law of the compressibility and the law of the dilatation of imperfect gases, and in which R_p and α_p change with the pressure. Thus we have for R_p the following values:—

	$p=$	0 metre.	0·76 metre.	1 metre.	5 metres.	10 metres.	15 metres.	20 metres.
Air	$R_p=$	29·222	29·325	29·347	29·672	30·007	30·265	30·446
Carbonic acid.	$R_p=$	19·329	19·388	19·437	20·417	21·907	23·867	25·915

But, according to M. Clausius, we have also $pv = \frac{u^2}{3g}$, g being the acceleration due to gravity, and u the mean velocity of the progressive motion; then

$$u = \sqrt{3R_p g(\alpha_p + t)}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

a formula which differs from that given by M. Clausius for perfect gases in that R_p and α_p are not constant, but functions of the pressure or volume. It may serve to determine the mean velocity of the molecules in the different gases. In the case of air and of carbonic acid, for which we have the requisite experimental data, we thus obtain the following velocities, expressed in metres per second:—

Pressure, in metres.	Air.		Carbonic acid.	
	$t=4°·8$.	$t=100°$.	$t=3°·3$.	$t=100°$.
0	485·1	566·9	393·3	459·7
0·76	484·4	566·9	392·1	459·2
1	484·8	566·9	391·8	459·0
5	483·8	566·9	385·0	456·4
10	482·8	566·9	374·5	452·8
15	482·0	566·9	362·9	449·4
20	481·4	566·9	350·4	446·2

The velocities found for the pressure zero represent the ideal case of a gas infinitely rarefied (that is to say, perfect), the attractions being infinitely small. We see that the velocities diminish when the pressure increases—that is to say, when the volume becomes small and the attractions are more intense. For atmospheric air at 100° it is necessary to carry the calculation to the second decimal place in order to find the differences, which shows clearly the degree of perfection that this gas reaches at that temperature. It seems almost superfluous to remark that, in order that the numbers given for air may have a real significance, we must consider air, not as a mixture of two gases, but as a single ideal gas whose molecules possess the physical properties of nitrogen and of oxygen in known proportions.—*Comptes Rendus*, July 12, 1869.

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XXXIX. *Observations on the Temperature of the Human Body at various Altitudes, in connexion with the act of Ascending.* By WILLIAM MARCET, *M.D., F.R.S., Assistant Physician to the Hospital for Consumption and Diseases of the Chest, Brompton**.

DURING an excursion over the Mont-Blanc range I had an opportunity this summer of ascertaining the temperature of my body under various circumstances connected with the act of ascending. The number of observations is, I must admit, much too small; still their individual results, when compared with each other, agree closely enough to allow of certain conclusions to be derived from them.

I had with me a thermometer carefully made by Casella, and divided in fifths of degrees Centigrade, allowing of a tenth of a degree to be read off. The instrument could be accurately observed while its bulb was under my tongue, by means of a small mirror which, on being placed near the stem of the thermometer at a certain angle, reflected its image into my eyes, so that I could see the mercury rising or falling as plainly as if I was looking at it directly†. It is useless to add that in

* Communicated by the Author, having been read to the "Société de Physique et d'Histoire Naturelle of Geneva" on the 3rd of September, 1869.

† On every occasion, I observed the height of the thermometer several times, and made sure of its being constant before noting the temperature, thus avoiding the fallacy which may easily arise from too short an exposure, as shown by Dr. Ch. Baeumler (Brit. Med. Journ. August 1869). Two observations made in London in the sitting posture, at 11 A.M. (breakfast at 9), with a thermometer constructed for me since my return by Casella,

these experiments the greatest care was taken to keep the bulb of the thermometer as far back as possible beneath the tongue, while the margin of that organ was applied firmly against the lower jaw, the lips being kept closed, so that respiration was entirely effected through the nose. It was, consequently, impossible that any of the air used for breathing could come into contact with the bulb of the thermometer while these observations were carried on.

The questions which offered themselves for investigation related (1) to the influence of various degrees of altitude on animal heat, the body being in a state of rest; (2) to the influence of the act of ascending on animal heat observed at different heights; and (3) to the influence of the act of descending on the temperature of the body. I shall limit myself, on the present occasion, to the first two questions, leaving the influence of the act of descending on animal heat for future consideration.

I soon ascertained the necessity of taking the temperatures while actually engaged in climbing; accordingly this was done. The instrument was withdrawn from its case and introduced under my tongue; the looking-glass was removed from the pocket, together with my watch; and the height of the thermometer was observed while in the act of ascending, and taking every care to slacken my speed as little as possible.

I began by noticing that frequently, while ascending, the thermometer after a sufficiently long exposure showed a temperature which, though steady at the time, commenced rising shortly

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as nearly as possible on the model of that which I had used, gave the following results; the bulb was kept under the tongue, and the degrees read off with a looking-glass :—

	minutes.		Temperature.
I. After an exposure of	1	....	36°·2 Centigrade.
,, ,,	2	....	36·5
,, ,,	3	....	36·7
,, ,,	4	....	36·8
,, ,,	5	....	36·8
,, ,,	6	steady at	36·9
,, ,,	7	....	36·9
,, ,,	8	....	36·9
II. After an exposure of	2	....	36·4
Next day under the same circumstances,	$2\frac{1}{2}$	....	36·5
,, ,,	$3\frac{1}{2}$	....	36·7
,, ,,	$4\frac{1}{2}$	....	36·8
,, ,,	$5\frac{1}{2}$	steady at	36·9
,, ,,	$6\frac{1}{2}$	....	36·9
,, ,,	$7\frac{1}{2}$	....	36·9

after (say a minute later), while I continued walking. The temperature, I conclude, was steady when first observed, as I could read it several times over without its altering; and the rising which took place a minute or two later was rapid, and would not have allowed of two similar readings being taken in succession. I explain this by assuming that, with the object of observing correctly the degrees of the thermometer, I was necessarily compelled to slacken my speed a little, thereby allowing heat to be formed afresh by the body to make up for that which had been used in excess in the act of climbing.

I made a rule, with every observation, to note the time when food had been last taken, and observed that walking up hill *fasting* cools the body to a greater degree than it does after taking food, or while digestion is going on. Thus on arriving at the "Pierre à l'échelle," 2060 metres, while in the act of ascending my temperature was 36°·5, and shortly after 36°·8. I made two excursions from Chamounix, with the object of determining the influence of food on the temperature of the body while going up hill. About an hour's climbing on the Brevent takes the tourist to a hut called the "Châlet des Chablettes." I left Chamounix about an hour and a half after a plentiful breakfast and while digestion was still going on; about a quarter of an hour before arriving at the châlet, and without slackening my speed, I ascertained my temperature under the tongue to be steady at 36°·5 after four minutes' exposure; and on walking slower, after having noted this result, the temperature rose to 37°. On one of the following days, I left Chamounix for the Châlet des Chablettes early in the morning before breakfast, and consequently having taken no food whatever since the previous evening. About a quarter of an hour before arriving at the châlet, and while keeping up the speed of ascent, I ascertained my temperature to be 35°·3, the bulb of the thermometer having been kept for six minutes under my tongue. The heat of the body in this last experiment, or while ascending with an empty stomach, was therefore 1°·2 less than in the previous experiment, when food had been taken. After showing 35°·3 in this last experiment, the thermometer rose rapidly, probably because I did not walk up quite so fast as I had done before reading the instrument; and on arriving at the châlet it was up to 36°·4.

I have thought it best to report my observations in the form of Tables, which are appended to this communication; the results obtained are as follows.

Result I.—That the temperature of the body, in the state of rest, does not, as a rule, appear to fall at increasing altitudes above the sea, and consequently a lessening of the atmospheric pressure

does not appear to have a marked influence on the temperature of man while at rest. Thus,

Temperature of the body at Chamounix, 1050 metres, before breakfast	36°·2
Same experiment another day	36·3
At the "Cabane des Grands Mulets," 3050 metres, before breakfast	36·5
At the summit of the "Col du Géant," 3362 metres, after eight minutes' rest and fasting	36·8
At the Grand Plateau (Mont Blanc), 4000 metres, at rest and fasting	36·3
On the highest "Bosse du Dromadaire" (Mont Blanc), 4672 metres, about 2^h 40^m after breakfast, and at rest	37·1

In twenty observations made while in the state of rest, at altitudes varying from 1050 to 4672 metres, and in various conditions as regards the food taken, the temperature of my body varied from 36° to 37°·1, or 1°·1 only; and it is remarkable that the highest temperature was found at the greatest altitude*.

Although there exists a comparative degreee of uniformity between the various temperatures observed when at rest, it is worth remarking that the highest can, as a rule, be connected with the circumstance that food had been taken not long previously, or with the fact that the thermometer had been observed while in the act of resting on the way down hill. The six highest readings of the thermometer, with but one exception, may be accounted for in that way; they are as follows:—

metres.	Temp. °	
1050	37·1	Chamounix, ¾ of an hour after breakfast.
3050	37·1	After arrival at the Grands Mulets about 2½ hours after a full breakfast with meat.
4672	37·1	Bosse du Dromadaire; breakfast with meat 2¾ hours before; then a steep and exciting ascent, but slow and without fatigue.
1621	37·0	Fasting, but down hill (Col du Géant), for four or five hours.
1565	36·9	Châlet des Chablettes, 1½ hour after full breakfast.
3362	36·8	Summit of Col du Géant, breakfast with coffee three hours before; temperature taken after eight minutes' rest.

This last observation, made at the summit of the Col du Géant, appears to be an exception to the rule: the temperature then

* An observation at Planpraz, showing a temperature of 35°·5, is not here taken into account; it is exceptionally low, which must be owing to some extraordinary circumstance, such as excessive perspiration during the last part of the ascent.

noted was very high, considering that there could be at that time no food in the stomach, and the ascent had been uninterrupted for the previous three hours. This was perhaps due to a reaction, the temperature having fallen very low (34°·5) during the ascent.

Result II.—That the temperature of the body during the act of ascending has invariably a tendency to fall, but that the degree of cooling depends mainly on a condition of fasting, or want of food, at the time. A rapid and steep ascent on an empty stomach, when the body is out of breath and perspiring freely, appears to be attended with the greatest reduction of heat.

In twelve observations made while walking up hill the temperature of the body varied from 34°·5 to 36°·5, the range being thus 2°·0. The greatest fall of temperature observed on four occasions was as follows:—

At 4000 metres	34°·5, fasting.
,, 2080 ,, ...	34·5 and 35°, fasting.
,, 3362 ,, ...	34·5, fasting.
About 2100 ,, ...	35·0, fasting.

The influence of walking up hill on the temperature of the body was well marked in the two following experiments:—

I walked up from Chamounix to Planpraz (Brevent) soon after an excellent breakfast and during digestion. When halfway up (at the "Châlet des Chablettes"), and after walking up hill for an hour, my digestion was hardly over, and I was free from the slightest sensation of fatigue; my temperature under the tongue, while ascending, was then much the same as before leaving Chamounix, being first 36°·5, and a few minutes after 37°. I then continued my way up to Planpraz, an hour's walk above the previous station. Being in a hurry to attain this spot, I took short cuts, climbing the face of the mountain, which was rather steep, and I reached Planpraz much out of breath and perspiring freely; the last few minutes before arriving, while walking, my temperature was 34°·5, and shortly afterwards 35°, say 1° lower than on reaching the "Chablettes." It was very obvious that my morning meal was then no longer able to make up for the loss of heat from the climbing which it had done an hour before; I had been moreover walking faster up a steeper hill than at first.

In the second experiment, in order to make sure that during the act of climbing the process of cooling (to which the body was subjected) was really due to muscular exercise and not to a change of altitude, I took a mule at Courmayeur for a part of the distance to the "Pavillon du Mont Fréty," an altitude of

2197 metres. When about two-thirds of the way up, I ascertained my temperature under the tongue to be 36°·4. I then got down and ascended on foot as quick as possible to the pavillon; this lasted 32 minutes, when on nearing my destination I ascertained my temperature to be 35° (after about five minutes' exposure), or 1°·4 lower than when leaving the mule. Shortly before starting from Courmayeur, an hour after dinner, the reading of the thermometer in my mouth was 36°·8; at about 650 metres higher up, while ascending on the mule and keeping very quiet all the time, my temperature was 36°·4; and after ascending about 328 metres higher up, walking fast, I had lost no less than 1°·4 of heat, showing the influence of walking in excess of that which could possibly be due to increased altitude*.

Taking 36°·6 as the average temperature at rest, according to my observations we have an average loss of heat of 1°·3 due to the act of climbing.

Result III.—That the temperature of the body, after falling while walking up hill, rises afresh very rapidly on resting, or on merely lessening the speed of ascent. Thus, a few minutes before arriving at the Pavillon du Mont Fréty, while in the act of ascending, the temperature under my tongue was 35°; after half an hour's rest at the pavillon it had risen to 36°·6, or 1°·6. As I was on the point of reaching the Col du Géant, while still ascending, the thermometer with its bulb under my tongue showed 34°·5; after remaining quiet for eight minutes on the summit of the pass, my temperature had risen to 36°·8, or 2°·3. Just before reaching the Chálet des Chablettes, while ascending, temperature after six minutes' exposure 35°·3; immediately after recording this in my note-book, although actually without stopping, the mercury rose to 36°, and in about five minutes later to 36°·4. I had evidently recovered my lost temperature during the interruption of the rate of ascending, owing to the act of taking the note.

The experiment at Planpraz was equally interesting. Just before arriving, while ascending rapidly and after four minutes' exposure, the temperature was 34°·5, remaining steady for about one minute; then having lessened my speed, the thermometer rose rapidly to 35°. During the first three minutes' rest at Planpraz the temperature increased again by 0°·8, and after about a quarter of an hour was steady at 35°·6.

I cannot explain the increase of temperature which occurred in the following observations, unless by assuming it to have been

* This experiment, however, should have been made by riding a mule up to a certain height, ascertaining the body-heat, and repeating the same ascent on foot, when the temperature should have been again determined.

due to the necessity of slackening my speed of ascent in order to record my observations.

At about 1565 metres above the level of the sea, the thermometer in my mouth, and while I was ascending, showed 36°·5, but rose to 37° during the next few minutes. In another experiment at the same place, although under different circumstances, the thermometer after five minutes' exposure was up to 35°·3, rising immediately after to 36°, and five minutes later to 36°·3, although I had not stopped walking up hill. At a height of 2060 metres, near "Pierre Pointue," while ascending, the temperature under my tongue, after five minutes' exposure, was 35°·5, rising during the subsequent few minutes' climbing to 36°·8. At the "Pierre à l'échelle," near the glacier "Des Bossons," while ascending and after an exposure of six minutes, the temperature first observed was 36°·5, and five minutes later 36°·8, although I had not stopped.

Result IV.—Finally, the sickness many people suffer from at great altitudes appears to be attended with a remarkable fall in the temperature of the body.

I suffered from this affection, for a short time, at the Pavillon du Mont Fréty, on awaking early in the morning. Immediately after an attack of retching, my stomach being then quite empty, I took the temperature under my tongue: the reading of the thermometer was steady at 35°; and the mercury rose slowly during the following few minutes to 36°, during which time I recovered my health perfectly. On arriving at the summit of Mont Blanc the same kind of sickness returned. I then attempted to ascertain my temperature, but while so doing had the misfortune to break my thermometer. Professor Lortet of Lyons, with whom I had the pleasure of making the ascent, then kindly lent me a maximum-thermometer, which he read after its bulb had been under my tongue for a short time; the instrument then showed a much lower temperature than I had ever yet observed; but the time of exposure was, I feel certain, too short for an accurate observation; still, after a similar exposure while in health, I believe the mercury would have risen higher. I can hardly think that in perfect health, and with no great degree of muscular exhaustion, the heat of the body at rest is much lower at the top of Mont Blanc than in the plain—and this for the reason that on the highest point of the "Bosse du Dromadaire," at an altitude of 4672 metres, and consequently only 138 metres below the very summit of Mont Blanc, my temperature when sitting down was 37°·1, which is certainly not below the normal temperature of man in the plain. I then felt in no way indisposed, and not at all tired.

I cannot help thinking that mountain-sickness is due to want of power in the body of recovering the heat it loses under those physiological circumstances to which it is subjected on mountains. At a certain height the body is altogether placed under cooling circumstances, such as cold weather and frequently insufficient clothing; at night there is often a deficiency of bed-clothes; and as a climber must be an early riser, he commences his day's work (after a cold night) precisely at the period in the twenty-four hours when, under ordinary circumstances, his body is coldest; food has often to be taken cold; and to this may be added the cooling process from muscular exertion in the act of climbing. In order to resist this cooling action, the vital energy ought to be proportionally high; it is so in many cases, but not always, either from exhaustion, or from a deficient supply of food, or from want of appetite to take it—insufficient food not only contributing to reduce the vital energy, but also depriving the body of the material on which this energy has to act in order to make heat.

The result of my experience is that the circumstances which are known to be productive of animal heat are those best calculated to cure mountain-sickness.

These may be considered—as, first, going down- instead of uphill, which is known by many sufferers to cure the sickness. On going down hill there is little or no muscular exertion, and consequently, it may be anticipated, no great expenditure of animal heat.

I suffered last year from mountain-sickness on Mont Blanc from the "grand plateau" to the top of the Mur de la Côte, but felt immediately relieved on going down, and was quite well shortly afterwards. On that occasion every circumstance under which I happened to be, combined to lower my temperature: I had started from the Cabane des Grands Mulets having taken little or no food; an intensely cold wind, many degrees below the freezing-point, was driving clouds of frozen snow into the face; hands and feet were benumbed; and I had gone up by the corridor, where the well-known want of air must have assisted in lowering every vital phenomenon.

Next, a violent attack of vomiting is often followed by immediate return of health. At first I could not possibly understand the reason of this remarkable fact, nothing being brought up from the stomach, which was invariably empty, showing that the sickness could not be due to indigestion; but on considering this circumstance I have come to the conclusion that, by increasing considerably the rate of the circulation, the retching caused a rising of the heat of the body.

Finally, if food can be taken on the sickness first coming on, it will be found very useful to arrest or relieve the illness.

The best precaution to take against mountain-sickness is obviously to eat plenty of good substantial food, and to repeat the meals at short intervals. Should the appetite fail, I think it best to endeavour to take a little food as often as possible.

Temperature of the Body at increasing Altitudes in the state of rest.

Height in metres. (1 metre = 3 feet $3\frac{3}{8}$ inches very nearly.)		Temp. under the tongue.
1050.	Chamounix, before breakfast	36°·2
1050.	Same experiment another day	36·3
1050.	Chamounix, immediately after breakfast ..	36·5
1050.	Chamounix, $\frac{3}{4}$ of an hour after breakfast ..	36·1
1050.	Chamounix, sitting on way down from Chablettes, fasting	36·7
1215.	Courmayeur, before dinner	36·6
1215.	Courmayeur, 1 hour after dinner	36·8
About 1320.	(Chamounix) Cascade de Blaitière, about $2\frac{1}{2}$ hours after luncheon	36·5
About 1565.	Châlet des Chablettes, before breakfast ..	36·3
,,	,, ,, after breakfast	36·9
About 1621.	Above Montanvert, after rapid descent, fasting	37·0
About 1869.	Ascent to Pavillon du Mont Fréty, riding. .	36·4
2080.	Planpraz, fasting (4 hours after breakfast).	35·6
2197.	$\frac{1}{2}$ an hour after arrival at Pavillon du Mont Fréty, fasting	36·6
2197.	Mont Fréty, after sickness, fasting	36·0
3050.	Cabane des Grands Mulets, resting, and about $2\frac{1}{2}$ hours after meal	37·1
3050.	Cabane des Grands Mulets, 2 A.M., before breakfast	36·5
3362.	Summit, Col du Géant, after 8 minutes' rest, and fasting	36·8
4000.	Grand Plateau, $4\frac{1}{2}$ hours after light breakfast.	36·3
4672.	Crête of the Bosse du Dromadaire, after 8 minutes' rest, last meal about 3 hours before on Grand Plateau	37·1
	Mean	36·6

Twenty observations.

Extremes $\left\{ \begin{array}{l} 37^\circ{\cdot}1 \\ 36{\cdot}0 \end{array} \right.$ omitting the experiment at Planpraz.

$\overline{1{\cdot}1}$

Temperature of the Body at increasing Altitudes during the act of ascending.

metres.		Temp. under the tongue.
About 1350.	Blaitière Waterfall, about 5 hours after breakfast .... (4 minutes' exposure)	5·5
About 1500.	Fasting (Chablettes) (6 minutes' exposure)	35·3
,,	Chablettes, after full breakfast .. (4 minutes' exposure)	36·5
2060.	Pierre Pointue, fasting .... (5 minutes' exposure)	35·5
2080.	Planpraz, about 4 hours after breakfast .... (4 minutes' exposure)	34·5
About 2100.	Under Pavillon du Mont Fréty, fasting (about 5 minutes' exposure)	35·0
About 2260.	Arriving at Pierre à l'échelle, in full digestion ...... (6 minutes' exposure)	36·5
3050.	Arriving at "Grands Mulets," fasting..	35·8
3362.	Arriving at Col du Géant, fasting .. (over 5 minutes' exposure)	34·5
3900.	Arriving at Grand Plateau (Mont Blanc), about 100 metres below, fasting.. (6 or 7 minutes' exposure)	35·6
4000.	Immediately on arrival at Grand Plateau, not walking, fasting (rose very rapidly to 36°·3)	34·5
4331.	Dôme du Gouter (Mont Blanc)......	34·6
	Mean..............	35·3

Twelve observations. Lowest temperature 34°·5.

XL. *On that portion of the Report of the Astronomer to the Madras Government on the Eclipse of August* 1868 *which recounts his Spectroscopic Observations. By* J. Herschel, *Lieut. R.E.**

THE instrument used by Mr. Pogson for this portion of his observations was of the same pattern, it is believed, as that used by the present writer. In the annexed Table the positions of Mr. Pogson's bright lines are fixed with all the accuracy at present attainable, by comparison with data in the writer's possession.

The first column indicates Fraunhofer's lines.

The second shows readings taken in the early part of 1868 with the Royal Society's spectroscope referred to above.

The third is deduced from the second by the empirical for-

* Communicated by the Author.

mula $86{\cdot}2\,H + 1440$, which refers the readings to another scale and zero.

In the following column are shown Mr. Pogson's readings of the solar lines, of which those *in italics* are *bright-line measures**. The dark-line measures correspond with converted measures in the previous column; and the close agreement shows that the empirical formula is correct, and that the dispersions of the two instruments are commensurable throughout. Mr. Pogson's dark solar lines are also unmistakably identifiable with B, C, D, *b*, F, and another, unnamed line, instead of those whose names he has assigned.

In the fifth column, those in the fourth are converted by the empirical formula $4{\cdot}40\,P - 6207$, which refers the readings to the scale and zero of that part of Kirchhoff's map in which the bright lines must be placed.

The sixth column is derived from the second by the formula $4{\cdot}40(86{\cdot}2\,H + 1440) - 6207$, or $379{\cdot}3\,H + 129$, and is merely a check on the identity of the lines supposed to have been measured.

The seventh column shows Kirchhoff's measures. Compared with the two previous ones, it is evident that the empirical formulæ by which they are obtained are only applicable strictly to a small portion of the spectrum—as was to be expected.

The last columns show the positions on Kirchhoff's scale of all the bright solar lines of which measures are available, with their (temporary) reference letters.

Mr. Pogson's data would be more valuable had the dark solar lines been measured *immediately* before and after the event; but he assures the writer that there was little, if any, change of zero to be detected. He is also quite confident of the accuracy of the bright-line measures.

It is very remarkable that the red line $H\alpha$ was quite unseen. Equally noteworthy is the evident preeminent brilliance of the green lines (measured) which he describes as *very* bright, although (owing perhaps to distressed eyesight) he was unconscious at the time of their absolute colour.

No green lines have yet been seen *here* with an uneclipsed sun, although $H\gamma$ is frequently seen. Unless, therefore, greatly increased dispersion can be brought to bear, future eclipses must be depended on for the identification of these lines, whose existence has been vouched for by four observers of the late memorable one.

* [*Sic* in MS. The italics indicated are evidently the measures corresponding to P_1 and P_2 in column 9.—J. F. W. H.]

TABLE showing the identification of Mr. Pogson's Solar Dark Lines, and calculated places of his Solar Bright Lines, and also the positions of the other known bright lines.

1. Fraunhofer's letters.	2. Measures with Royal Society's spectroscope. H.	3. H′= 86·2 H +1440.	4. Mr. Pogson's measures. P.	5. P′= 4·40 P −6207.	6. H″= 379·3 H +129.	7. K.	8. Positions of known bright lines on Kirchhoff's scale.	9. Reference letters.
B	0·86	1514	1513	......	.....	593		
							655 ??	ϵ
C	1·25	1548	1547	.....	603	694	694	$H\alpha$
D	2·30	1638	1639	1005	1001	1005		
							1014–5	δ
	2·85	......	.....	....	1210	1207		
			1743	1462	......	......	1462 ??	P_1
	3·52	......	......	......	1464	1463		
E	3·68	......	......	......	1525	1523		
			1763	1550	......	......	1550 ??	P_2
b_1	3·97	1782	1782	1634	1635	1634		
			1784					
	4·65	......	......	......	1893	1909		
	4·79	.....	......	......	1946	1961		
F	5·03	1873	1873	......	2037	2080	2080	$H\beta$
							2596 ?	
	7·10	2052	2055	......	......	2721		
							2796	
G	7·55	......	......	......	......	2855		

1. The accordance between H′ and P proves the commensurability of H and P throughout.

2. The accordance between H″ and K *from* D *to b* is a measure of commensurability of H and K, and ∴ of P and K *within those limits.*

3. Therefore P′ *within those limits* is equivalent to K.

Bangalore, August 30, 1869.

XLI. *Short Account of the Winterings in the Arctic Regions during the last fifty years. By* C. BÖRGEN *and* R. COPELAND, *Astronomers and Physicists to the second German Polar Expedition*.*

AT the present moment, when it is intended to send out a second expedition to the arctic regions from Germany with the purpose of wintering there, it may not be uninteresting to give a short historical review of the winterings which have

* Translated by W. S. Dallas, F.L.S., from Petermann's *Mittheilungen,* 1869, pp. 142–154.

been effected during the last fifty years. The precautions which were found useful in these, the number of deaths and accidents, the occupations and scientific operations will be particularly indicated, in order to show how unfounded is the opinion still frequently entertained by the general public that it is impossible for Europeans to endure the winter in those climates, and at the same time to lay down more accurately the scientific operations which may be carried out during the winter.

The first wintering of an exploring expedition of which we have any knowledge is the unfortunate one of Sir Hugh Willoughby in the year 1553, who, being cast away by a storm, was frozen in upon the coast of Lapland, and perished by hunger and cold with his whole crew.

This melancholy occurrence did not, however, deter other bold seamen from repeatedly making the attempt to discover a commercial route north of Europe and Asia to the fabulous kingdom of Cathay; and by these expeditions Spitzbergen, Nowaja Semlä, &c. first became known to western Europeans.

One of these expeditions sailed from Holland in the year 1596; its conduct was entrusted to Jakob Heemskerk and his truly wonderful pilot, William Barents. Their ship was beset by the ice on the north-east coast of Nowaja Semlä, and they themselves compelled to pass the winter on that inhospitable shore. Of the crew, which consisted of seventeen persons, five died—two during the residence on Nowaja Semlä, three during the return voyage, among whom was Barents; all of them suffered more or less from scurvy. Nevertheless this wintering must be regarded as a very successful one for that time; and even to the present day our entire knowledge of the north and north-east coasts of Nowaja Semlä is founded upon this voyage, as no one, since Barents, has succeeded in reaching the "Eishafen" where he wintered.

Many attempts have subsequently been made to pass the winter in the arctic and otherwise uninhabited regions, upon Spitzbergen, Jan Mayen, and in the Hudson's Bay Territories, but of these unfortunately by far the greater part were failures. The causes of this in most cases were scurvy and the necessity, owing to the want of sufficient clothing, of keeping too carefully shut up in the huts. We must, however, admire the courage and steadfastness of these people, who exposed themselves in such complete dependence upon good luck to the inclement climate, and at the same time, with the greatest perseverance, so long as the hand weakened by illness could barely guide the pen, continued to write in their journals, in which they described the course of the weather and the conditions of temperature.

Successful winterings are, however, to be noted even among these, and indeed one in which this was hardly to be expected. In the year 1630, eight sailors belonging to an English whaler were separated from the ship and compelled to pass the winter on Spitzbergen under 77° N. lat. Of course they had no provisions from the ship, and we might therefore have anticipated that they would not live through the winter. But this very circumstance was their salvation; for in order to obtain nourishment they were obliged to go hunting, and were fortunate enough to kill a sufficient number of reindeers and bears to furnish them with fresh meat and warm clothing. The fresh meat, in conjunction with much moving about in the open air (the two conditions of health in this climate), kept them strong and healthy, and thus they were found and brought home in May of the following year by their former ship, without any of them having been seriously ill during the winter.

But unfortunately such a successful wintering as this was at that time an exception; and it is therefore no wonder that fifty years ago the opinion was still entertained that it was impossible for Europeans to pass the winter safely in the arctic regions. In the present day we may certainly say that at that time, and with the equipment in provisions and clothing then supplied, a wintering was attended with great danger to life; but that it is now no longer perilous has been sufficiently proved by the recent voyages.

For more than two centuries the idea of a "north-west passage," north of America from the Atlantic to the Pacific Ocean, as a commercial route to the East Indies and China, produced a series of English expeditions which led to the exploration of Hudson's and Baffin's Bays, to the discovery of Lancaster, Smith's, and Jones's Sounds, &c. But they showed at the same time that, if a north-west passage really existed, it was not fitted for commercial purposes. Hence, after Cook, in his last voyage in 1779, had made an attempt to penetrate through Behring's Straits, these voyages, which were commercially useless, were given up, and people contented themselves with working the rich fisheries found on the previous voyages of discovery.

For nearly forty years voyages of discovery towards the north ceased, until in 1815 Kotzebue made a fresh attempt to force the north-west passage from Behring's Straits. He got no further, however, than to the sound which is named after him. Now also a series of attempts was again made on the part of the English, to discover the north-west passage. But the object was now no longer to find a commercial route to China, but rather

to explore the wide unknown regions to the north of America, to determine how far the continent extended towards the pole, or whether islands lay off the coast, &c.

As the first of these voyages, we must name that undertaken in 1818 by Sir John Ross. Properly speaking, he only repeated the voyage made two centuries previously by Baffin, but did not consider it advisable to penetrate any further than the latter, and returned to England in the autumn of the same year, after making the rich fisheries in Lancaster Sound and Pond Bay accessible. If, therefore, this voyage did not essentially advance discovery, it nevertheless opened up a perfectly new region for the fishery in these waters.

The next expedition which sailed from England, well equipped scientifically and indeed with the intention of wintering, was sent out in the following year under Parry*, who had accompanied the preceding expedition under Ross. As this is the first wintering of a scientific expedition that produced valuable results, and the leaders of all subsequent voyages having guided themselves by the observations collected in it by Parry, we may be allowed to consider it somewhat in detail.

The expedition consisted of two ships, the 'Hecla' and 'Griper,' the former of 375, the latter of 180 tons burthen; the crews respectively of 51 and 36 men, officers and sailors together. On the 15th of May Parry left Yarmouth Roads, and on the 4th of September passed the 110th degree of longitude west of Greenwich, which had been appointed by the Admiralty for the gaining of a prize of £5000. He wintered in Melville Island, in the place named by him "Winter Harbour," under 110° 48′ 29″·2 W. long. and 74° 47′ 19″·4 N. lat.; but in the summer of the following year by a land expedition he attained 113° 48′ W. long., halfway between Baffin's Bay and Behring's Straits.

The expedition was equipped for two years, and especially well-furnished with the known antiscorbutic materials, such as dried vegetables, sauerkraut, pickles, vinegar (partly in a very concentrated state), lemon-juice with sugar &c., as also with preserved meat, all of the best quality and packed in air-tight vessels. Instead of bread a large stock of carefully dried flour was taken, so that fresh bread, baked on board, could always be had.

* Journal of a Voyage for the Discovery of a North-west Passage from the Atlantic to the Pacific, performed in the years 1819–20 in H.M.SS. 'Hecla' and 'Griper' under the orders of William Edward Parry, R.N., F.R.S.: London, 1821. And Supplement to the Appendix of Captain Parry's Voyage for the Discovery of a North-west Passage in 1819–20, containing an account of the subjects of Natural History: London, 1824.

These precautions proved to be extraordinarily beneficial to the health of the wintering party. The sick-list of the surgeon, Dr. Edwards, usually bore only one, or at the utmost two names of people who had slight attacks of scurvy; and these were cured in a few weeks by the administration of an extra dose of lemon-juice with sugar. On one occasion, however, when a fire broke out in the observatory, a considerable number (sixteen) of the people suffered a good deal from frost, as in their excitement they had neglected the necessary precautions; and this led in some cases even to the amputation of fingers. The expedition had only one death to lament; and this was caused by disease of the lungs, which became combined with scurvy. The sanitary condition of this wintering was therefore excellent, thanks to Parry's indefatigable care and its admirable equipment.

The ships were laid up for wintering in the following manner; but it is to be observed that in subsequent winterings these arrangements were altered and improved in some few particulars, which will be noticed hereafter. The moveable ropes and yards were taken down. The former were left lying in the open, where they froze quite hard, and in this state were completely protected from rotting, to which they would have been exposed in the moist air between decks.

The entire deck was then provided with a high-pitched roof of oil-cloth, and served during the winter, in bad weather, as an exercise-ground and promenade for the officers and men. At first Parry had the water kept open around the ships, until he found that this would be too troublesome. Then he allowed the ships to be frozen in, and had snow shovelled up against their sides in order to keep in the heat; and this at the same time had the great advantage that the ice round the ship did not become so thick as where no snow covered it.

The greatest evil that Parry had to contend against was the great amount of moisture in the cabins, which in some cases reached such a pitch that the beds were one half frozen, and one half completely wet through. At first the ice condensed on the walls was removed daily; and once when this had been omitted for some weeks, no less than 5000 or 6000 pounds of ice were taken out of the cabins. Twice a day, when the crew were abroad, their quarters were examined by the commander and the surgeon; and in general the actual observance of the precautions was most rigidly watched by the officers: thus, for example, the people were obliged every day to take the prescribed quantity of lemon-juice and sugar in the presence of one of the officers. The dampness was very much increased by the circumstance that Parry was obliged to have all the clothes washed during winter dried

between decks. The fixed berths, which had been introduced into the ships quite against the ordinary practice of a man-of-war, had to be exchanged for hammocks, entirely on board the 'Griper,' and partially on board the 'Hecla;' and this (from the great amount of moisture) contributed greatly to the maintenance of good health; nay, one officer, whose life was considered in some danger, was thereby completely restored in a few weeks.

That the cabins could not be cleaned with water under such circumstances was a matter of course. Instead of this the floors were scrubbed with stones and hot sand which had stood all night upon the stove.

All these precautions would not, however, have sufficed for the preservation of health if the people had not played and been exercised in the open air for several hours daily. Hunting parties obtained a provision of 3766 pounds of fresh meat, which formed a welcome addition to the stock of provisions, leaving out of consideration the good effect of movement upon the health. To keep up their spirits, which might well evaporate even from the boldest heart during the long polar night, a weekly journal was edited by Captain Sabine (now General Sabine, and President of the Royal Society), which contained articles of a mixed, serious and lively character; and a theatre was set up on which some small piece was acted every fortnight; and this was carried on with so much zeal that even a temperature of $-2°$ F. ($-15°$ R.) upon the stage did not deter the improvised actors from contributing to their own and their companions' amusement.

That the scientific objects of the expedition were not at the same time neglected is proved by the long series of observations and investigations which are appended to Parry's report, and of which we shall shortly have to speak more in detail.

As a precaution in case of fire, a hole was kept open in the ice near the ships; but this fortunately was never required; for the observatory, in which a fire broke out, was at a distance of 2100 feet from the ships, and must therefore have been extinguished in some other manner, during which operation, as already mentioned, sixteen of the people suffered a good deal from frost.

As regards scientific results, we must mention in the first place the discovery of Barrow's Strait, and the opening up of an extent of coast of 35° of longitude, which subsequently proved to be the south coast of a series of islands; and towards the south the existence of a broad strait (Prince-Regent Inlet) was ascertained, which was further investigated by Parry on a subsequent voyage. On the return voyage the whole east coast of Cockburn's Land, extending for 8° of latitude, was surveyed.

Here Sabine commenced his pendulum-experiments for the determination of the figure of the earth, which have since been continued with so much success and completeness; he also determined the magnetic constants of various points by very extensive observations. To the meteorology of the arctic regions the expedition devoted a series of observations continued uninterruptedly for twelve months between the parallels of 74° and 75° N. lat. The geographical position of Winter Harbour was established by the enormous number of 6862 moon-distances and 39 meridian altitudes. Tidal observations were regularly made; and, further, no fewer than fifteen chronometers, partly taken for the purpose of being tested, were examined as to the uniformity of their rates. Zoology and botany found in Dr. Edwards a zealous representative, who, with the assistance of Sabine, Parry, and James Ross, brought back a rich collection of specimens belonging to the animal and vegetable kingdoms, among which were several previously unknown species. At the same time he fulfilled his important duties as surgeon with the greatest zeal and care; and to his exertions and ceaseless watching of the sanitary condition the small number of cases of illness and death during the winter is mainly to be ascribed. This voyage, which laid down the rules for all subsequent wintering expeditions, was also scientifically the richest of all in results. It was followed by two other voyages of Parry's, one of them in the years 1821–23, in which two winters were passed in the arctic regions with equally favourable results with regard to health as in the first case*. The two winterings were performed exactly in the same fashion as in the preceding voyage; it would therefore lead only to unnecessary and tedious repetitions if we were to describe the ship in its winter harbour &c. In fact Parry himself says that we cannot easily imagine two things possessing more resemblance to each other than two winters in the higher latitudes of the arctic regions.

The first of the two winters was passed by Parry in Lyon's Inlet. He proved in it that Melville Peninsula is united to the mainland of North America, whereas it had previously been supposed that there was in this region a passage to Prince-Regent Inlet. Intercourse with the Eskimos during the winter furnished him with much important information as to the configuration of the land, and the existence of a great extent of open water in the northwest. Subsequent investigations showed the correctness of this and of many other geographical statements of the aborigines. In

* Journal of a Second Voyage for the Discovery of a North-west Passage, performed in the years 1821, 1822, 1823 in H.M.SS. 'Hecla' and 'Fury,' under the orders of W. E. Parry, R.N., F.R.S. London, 1824: Murray.

the following year only a small advance towards the north was made, and the winter was passed in Iglulik, when the Fury and Hecla Straits were discovered and examined during the winter by Parry's officers, who actually obtained a sight of the great sea of the Eskimos as a large surface covered with ice, which was afterwards known as the Gulf of Boothia.

After this second successful wintering, Parry returned with his two ships in good condition to England, having furnished, by passing two consecutive winters in the arctic regions with very little loss of life, a proof that it was very possible for Europeans to dwell in winter in those latitudes.

In the following year (1824) Parry sailed again for the discovery of the north-west passage, having set before him for this purpose the examination of the great passage, Prince-Regent Inlet, which had been observed on his first voyage. Being detained by the unfavourable condition of the ice in Baffin's Bay, Parry was compelled to winter in Port Bowen, a small harbour on the east coast of Prince-Regent Inlet. Here he had the misfortune of having one of his crew drowned.

He examined by land the west coast of Cockburn's Land, from his winter-harbour southwards to 72° N. lat., and northwards to Lancaster Sound. In the summer of the following year Parry went to the other side of Prince-Regent Inlet and investigated Creswell Bay, but lost his ship the 'Fury.' With his usual foresight Parry had the provisions and the extra stores of clothing brought on shore and enclosed in a wooden house built for this purpose. This depôt was of incalculable value to subsequent expeditions; and the stores assisted the last Franklin-expedition under M'Clintock, as much as thirty-three years afterwards, to complete their own equipment.

The land and coast expeditions in the north of America, carried out before 1830 by Richardson, Franklin, and Beechey, were obliged to winter under very different conditions; and as we have here chiefly to show what has been attained by means of ships, and how the dangers of the arctic winter may be diminished in *naval* expeditions, they need not be taken into consideration. It is sufficient to say that, with enormous toil and the loss of many men, they discovered and surveyed the north coast of North America from Cape Turnagain in 109° W. long. to Return Reef in 148°.

The next great naval expedition was undertaken by Sir John Ross in 1829*. It was fitted out by Sir Felix Booth, a rich merchant; and Ross desired by this voyage to reestablish his fame

* Narrative of a Second Voyage in search of a North-west Passage, and of a residence in the Arctic Regions during the years 1829, 1830, 1831, 1832, 1833. by Sir John Ross, Captain in the Royal Navy. London, 1835.

as a discoverer, which since 1818 had been frequently and violently attacked. In this he and his nephew, Sir James Clarke Ross, who has since become so famous by his antarctic voyages, most perfectly succeeded.

The expedition sailed from London in a paddle-steamer (the 'Victory') of 150 tons, with a crew of twenty-three persons, officers and men. The engine soon proved to be quite useless; and after a stoker had unfortunately lost his arm by means of it, and some unsuccessful attempts to employ it had been made, it was given up and finally disembarked at Fury Point (where Parry lost his ship). The unfortunate stoker had been left behind on the coast of Scotland and replaced by another.

Ross sailed through Lancaster Sound into Prince-Regent Inlet and wintered in Felix Harbour in 69° 58′ 42″ N. lat. and 92° 1′ 7″ W. long. On landing the engine, he took some provisions from the store left by Parry at Fury Point, so that at the beginning of the winter he was completely provisioned for two years and ten months. In arranging the ship for the winter, Parry's precautions and experiences served in general as a guide; but Ross introduced the essential improvements of covering the whole deck with snow, and establishing condensers for the purpose of keeping the space between decks dry. The latter were large metallic vessels turned upside down over openings of several inches in diameter made in the ceilings of the cabins. They were covered with snow, and the moist vapours arising from the space below were condensed in these cold cupolas, so as to prevent all moisture below the deck; the ice collected in them was removed weekly, when it amounted on an average to 500 or 600 pounds.

For the entertainment of his little crew a school was established, and otherwise the time was passed as in Parry's expedition. By frequent journeys in the summers of 1830 and 1831, James Clarke Ross investigated the two coasts of Boothia Felix, and ascertained that this land was connected with the American continent by the Isthmus of Boothia. On one of these journeys he reached the magnetic pole. Frequent intercourse with the Eskimos, who here again displayed great knowledge of their native country, gave him information of a large open water still further to the west (Victoria Strait)—just as Parry, when on the other side of the Melville peninsula, had heard much of the Gulf of Boothia, which was now cleared up by Ross. The natives even mentioned to him the subsequently discovered Bellot Strait which unites Prince-Regent Inlet with Franklin's (Peel's) Strait*; but when he examined the place described by them, the strait, which was concealed by several islands lying in front of it, escaped his observation, and he regarded the indentation of

* *Op. cit.* pp. 299 & 338.

the coast as a bay (Brentford Bay). The second winter he was obliged to pass nearly in the same place where he had remained during the first winter; and he then had to decide upon wintering for the third time quite close to his previous winter quarters, in Victoria Harbour. At the end of May 1832 he was obliged to quit his ship (the 'Victory') and to endeavour to save himself by means of sledges, taking his boats with him. They reached Fury Beach and afterwards Batty Bay. In this retreat Parry's precaution of bringing the 'Fury's' stores on shore saved the brave band from starvation. After pressing on to Batty Bay, Ross was surprised by the winter, and compelled, in order to save the lives of himself and his men, to return to Fury Beach. Here, in a house built of planks and coated with blocks of ice, they continued, by means of good stoves, to provide themselves with a comparatively warm and comfortable dwelling.

In the following summer they at last succeeded in reaching Barrow's Strait, and thence they sailed on in their boats and were taken up at the entrance of Lancaster Sound by the 'Isabella,' which had been sent to their assistance.

As regards the health of this expedition, we may say that in the first two winters it was very good. In the winter of 1829–30 Ross lost only a single man, who had concealed a disease of the lungs which had previously brought him several times to the hospital. No scurvy made its appearance. The first case of this disease occurred on the 20th of November, 1831, consequently at the beginning of the third winter, and it carried off two men.

When the expedition at last returned to England, after an absence of four years and a half, the crew was naturally in a very low state, and one of them died after the return to England in consequence of the hardships he had undergone; but nevertheless it must be a matter of wonder that no more fatalities occurred during so long a sojourn.

Again there was a period of twelve years during which all expeditions for the discovery of a north-west passage ceased. But, much as had been done in the exploration of the arctic regions of North America, there was still much to do before these regions could be regarded as even tolerably well known. The question as to the theoretical or practical possibility of a north-west passage was still unsolved; and the Government, finally yielding to the pressing instances of the Secretary to the Admiralty, Sir John Barrow, and to public opinion, ordered the ships 'Erebus' and 'Terror,' which had just returned from the antarctic expedition under Sir James Clarke Ross, upon a new voyage of discovery in the regions already so frequently visited, and conferred the command upon Sir John Franklin.

The unfortunate termination of this expedition is well known. Although the history of the last desperate attempt to escape contains many doubtful and unexplained points, we may obtain much information upon the earlier part of the expedition from the short report which was left on King William's Land by Crozier and Fitzjames, and discovered by Lieutenant Hobson, who accompanied the last searching expedition under M'Clintock.

The portion of this short report which is particularly interesting to us relates to the number of deaths, and runs as follows:—

"25 April, 1848. . . . . Sir John Franklin died on the 11th of June 1847, and the total loss by deaths in the expedition has been to this date nine officers and fifteen men." When the expedition sailed in the summer of 1845 the entire crew consisted of 129 people, officers and men, deducting the few who were sent back from Baffin's Bay on account of illness. The provisions were calculated for three years; but unfortunately a great part of them was supplied by the marine purveyor Goldner, who sought by the most shameful fraud to make a fortune, and filled the preserved-meat cases with completely useless offal instead of with eatable materials. By this means the provision was considerably diminished; but as Sir John Franklin wrote from Baffin's Bay full of hope that, if necessary, he should be able to hold out for five or even seven years by renewing his stores from the produce of the chase, we may assume that, notwithstanding the loss of what was useless, the provision was sufficient for three years in case of need.

The ships were abandoned in April 1848; and we may suppose that want had not then reached any very high degree. Up to this moment the expedition had hardly been in any worse position than that under Ross, for example, after the same lapse of time; and the number of deaths reported up to this period, although doubtless considerable, is by no means very surprising, especially when we consider that three of them occurred as early as the first winter (1845–46), on Beechey Island. What became of the 105 who were still living after the abandonment of the ships, will probably always remain in obscurity.

The apprehensions as to the fate of Franklin and his companions gave rise to a long series of searching expeditions, which are known in the history of arctic voyages as the Franklin-expeditions. To go through all the numerous expeditions singly would lead us too far. In M'Dougal's account of the voyage of the 'Resolute' in the years 1852–54*, there is an account of the numbers of the crews who wintered and the deaths which

* The eventful Voyage of H.M. Discovery Ship 'Resolute' to the Arctic Regions in search of Sir John Franklin, by George F. M'Dougall (London, 1857), p. 498.

occurred during the winterings. The following are English expeditions:—

	Crews.	Deaths.
1848–49, Sir James Clarke Ross .	138	7
1850–51, Captain Austin	180	1*
1850–51, Captain Penny	46	
1849–50, Mr. Saunders	40	4
1850–54, Captain M'Clure . . .	66	5†
1852–54, Sir E. Belcher	90	2
1852–54, Captain Kellett . . .	90	4‡
1852–54, Commander Pullen . .	40	

The great scientific results of these expeditions, and especially the enormous extent of coast which was explored by them, are well known. In the first place, towards the north, Smith Sound was investigated by Kane; and the coasts of Wellington Channel and the entire north coast of Parry Island were examined by Belcher. M'Clure penetrated from Behring's Strait through Investigator Sound, wintered three times in Banks's Land, and once, when he was obliged to abandon his ship, on Melville Island with Kellett; and he was the first who demonstrated the existence of a north-west passage by his actually tracing water-passages from Behring's Strait to Baffin's Bay, although these were in part impassable for ships. Kennedy and the French officer Bellot, who attached themselves to the expedition as volunteers, discovered Bellot's Strait, named after the latter, explored Prince-of-Wales's Land on the further side of Franklin's (Peel's) Strait, and returned northwards round North Somerset to their winter harbour in Batty Bay.

This is the longest sledge-journey that has been undertaken during the arctic explorations; its entire length amounts to 1200 nautical miles; and it was performed without any depôts for the return journey. Of his crew of eighteen men Kennedy did not lose one, and he had only a few quite unimportant cases of illness. He succeeded in bringing his little vessel ($89\frac{3}{4}$ tons) back to England in safety.

M'Clintock, in Austin's expedition, gave a quite unprecedented development to sledge-journeys; he improved the construction of the sledges and the mode in which the depôts were

* Sickly from the first, and died in consequence of hardships on sledge-journeys.

† All the deaths in the last year, from scurvy.

‡ One from disease of the heart, two from weakness in consequence of hardships, and one upon a sledge-journey.

It is unfortunately not stated, *l. c.*, what the causes of death were; and only in the cases here cited in the notes are we able to give any account of them.

thrown out; and it was only by means of these improvements that the important results were secured.

The principal service done by this expedition was the enlargement of our geographical knowledge of these regions, which, indeed, was the necessary consequence of its object. All its endeavours were directed to one end, namely the discovery of Franklin or of his traces; and hence it follows, as a matter of course, that whatever was not connected with this must have been regarded as a subsidiary matter.

Among the searching expeditions the two Grinnell expeditions were of scientific importance, and also very instructive in other respects; they were fitted out by a New York merchant named Grinnell, and accompanied by Dr. E. K. Kane.

The first of these expeditions* left New York on the 22nd of May, 1850. It consisted of the ships 'Advance' and 'Rescue,' and was under the orders of Lieutenant de Haven, who himself commanded the 'Advance,' whilst the 'Rescue' was commanded by Griffin. In the 'Advance' was the most important person of the company in a scientific point of view, Dr. Elisha Kent Kane. The crews of the ships, which were of 144 and 91 tons, consisted in all of 17 and 16 men. Their equipment was rather hastily performed; and hence there was no superfluity, especially of antiscorbutic agents. Kane himself, who was stationed in the Gulf of Mexico, received the order to take part in the expedition only two days before its departure, and had only forty hours in New York to look after his personal equipments and procure some scientific instruments; the latter, however, unfortunately were not put on board.

They reached Beechey Island in good time, and in conjunction with the English expeditions under Austin and Penny, which were there at the same time, undertook the investigation of Beechey Island, where the first certain traces of Franklin's expedition were found; they then made their way into Wellington Channel and discovered Grinnell Island. When they were then, in accordance with their instructions, about to return to New York, they were beset by the ice, and carried with it through Lancaster Sound and Baffin's Bay into the Atlantic Ocean. During this process they had to undergo many dangers and hardships; and the hasty and insufficient equipment now revenged itself upon them bitterly. It was only through the almost superhuman exertions of Kane, who, although himself ill, tended his companions in suffering with a truly affecting solicitude, that there was no loss of life to be lamented. He not only cared for the medical treatment of his patients, but brought from his hunting expedi-

* The United States Grinnell Expedition in search of Sir John Franklin, by E. K. Kane, M.D., U.S.N. London and New York, 1854.

tions much fresh meat into the ship, which did much good to the sick. But they had not only to suffer from scurvy; the cold also could not be sufficiently kept off. The ship was lifted so high upon the ice that it was impossible to heap the sides with snow or to adopt other customary precautions. It contributed not a little to heighten the difficulty of their position, that the 'Rescue' got into so bad a situation that she had to be abandoned for a time and her crew transferred to the 'Advance.'

Notwithstanding his heavy medical duties, Kane did not neglect to do whatever lay in his power for science. His report contains very many important notices upon the formation and movement of the arctic glaciers, with hints as to the deficiencies which still remain to be filled up in this field, and upon the peculiar ice-structures which occur here and there.

He complains that the confined space and overloading of the ship did not allow him to be so regularly active as he desired in scientific matters. The observations of temperature are irregular, but still very numerous; and in connexion with them he calls attention to various points, to precautions which must be employed in order to obtain correct readings, and to many other things. The northern lights found in him a zealous observer; and here also it did not escape his acute mind how much still remains to be explained in the theory of these phenomena.

The second voyage*, which was commanded by Kane himself, was fitted out by the two merchants, Grinnell of New York and Peabody of London, and its object likewise was to search for Sir John Franklin. The 'Advance' was again the abode of Kane and his little crew, seventeen in number, to whom a native (Hans Christian) was afterwards added. This vessel was a sailing brig of 140 tons, and had proved on the previous voyage to be a good ship for the ice. The equipment consisted of india-rubber tents, sledges of the newest construction, and provisions consisting of 2000 lbs. of pemmican, bread, flour, dried fruits, preserved vegetables, &c., and besides these a considerable quantity of salted meat, which had better have been left behind. As a scientific equipment, there were on board a large library and a valuable stock of instruments.

Kane selected Smith Sound for his base of operations, as he had explained in a memoir read before the Geographical Society. From this he proposed to push towards the north. That he could find nothing there relating to Franklin's expedition appeared clearly enough from the subsequent discoveries; but he penetrated far to the north, surveyed the shores of Smith Sound

* Arctic Explorations.—The Second Grinnell Expedition in search of Sir John Franklin, 1853, 1854, 1855, by Elisha Kent Kane. 2 vols. Philadelphia, 1856.

and Kennedy's Channel as far as 81° N. lat., and discovered the enormous Humboldt glacier, which extends more than a degree in width. He was obliged to remain in Rensselaer Bay, in 78° 37′ N. lat. and 70° W. long., where he passed one winter, which threw many of his companions and himself upon a sick bed. Nearly all had scurvy; and the fatiguing sledge-journeys were by no means adapted to improve the health of the expedition. Upon one of these journeys, made by some of the crew in order to establish a depôt of provisions, they were beset by the ice, and would have been destroyed if Kane had not relieved them; he could not, however, prevent two of them from dying in consequence of the fearful hardships. Hunting did not furnish any very considerable results; and feeling certain that they would be set free in the following summer so as to return home, they were not so economical in the use of what was procured by the chase as they might perhaps have been. But the summer brought them no release, and they were compelled to hunt for their provisions until the next winter, but, unfortunately, with small results. In one of their very distant hunting expeditions, which was led by Morton and the Greenlander Hans, they reached in 81° N. lat. a coast which was washed by a sea perfectly free from ice and with long regular dunes.

Dr. Hayes, who was making his first arctic voyage, discovered Grinnell Land, and, besides fulfilling his medical duties, which were in themselves great and heavy enough, made many journeys for the purpose of hunting and exploring, in which he was assisted by the astronomer, August Sonntag.

The second winter was long and severe, and brought with it many hardships and much suffering; diseases, especially scurvy, combined with cold and hunger to put the courage and steadiness of the explorers to the hardest test. As the second spring again failed to set them free, they were forced to adopt the desperate expedient of seeking inhabited regions in small open boats. After infinite exertions, which cost one of them his life, they reached Upernavik, and were afterwards taken up in Godhavn by the expedition under Hartstein, which had been sent to seek for them.

Notwithstanding the many difficulties and hardships with which this expedition had to contend, its scientific results are by no means inconsiderable. Observations of temperature, to which Kane attached great importance, were made hourly during the voyage, but showed at the same time how careful it is necessary to be in such cases in order to avoid the influence of the warm ship, which is observable at a distance of several hundred paces. They showed further the untrustworthiness of the spirit-thermometer at low temperatures; the eleven thermometers

which were constantly read differed at a temperature of $-68°$ F. from the mean of all readings by no less than $12°$; the difference increased from $-20°$ F. downwards, at which temperature it varied between $-1°{\cdot}2$ and $+1°{\cdot}2$ for the different thermometers.

The mean temperatures, compared with those obtained in other parts of the arctic regions, furnish interesting data for the comparison of the climates, and show that the climate of Greenland, from being an insular climate in the south, approximates towards the north to the coast climate of the arctic archipelago in the west of Baffin's Bay, the character of which is not far from that of a continental climate. We shall have to speak more in detail upon this point hereafter. Magnetic observations were made in great numbers by Sonntag; and during the winter of 1854–55 six magnetic terms of 24 hours each were kept, the results of which are to be found in the appendix to the Report, which also contains a long list, with descriptions, of the plants and animals collected by Kane upon the two expeditions.

We have already mentioned Kane's voyages as very instructive in every respect; and they are especially instructive negatively, inasmuch as they show the dangers to which arctic voyages are exposed when the greatest care is not employed in their equipment. If instead of the salted meat he had had some 1000 pounds more pemmican, he would certainly not have had to undergo such terrible want and suffering. He regarded the salt meat as so useless and so injurious to those who were ill of scurvy, that in sending out a company to bring in the provisions stored in a depôt, he gave the strictest orders that all salted meat should be left behind, and this at a time when the expedition was in danger of dying with hunger.

It was a modest desire to spare as much as possible the means of the high-spirited men who fitted out the expedition, and a certain expectation that he would be able to return after the first winter, that induced Kane not to provide himself with stores of better quality and for a longer time, although he had undergone similar experiences on his first voyage. Far be it from us to wish to reproach him with this; his courage and perseverance, and his remarkable management and scientific activity, in which he far surpassed all previous arctic voyagers, place him in the first rank of travellers, and the smallness of the loss of human life which this expedition had to regret is to be ascribed solely to his medical skill and persevering care. The hardships of this second expedition threw the brave man, soon after his return, upon a sick bed, from which he was never again to rise.

His reports upon the two journeys are full of hints upon the arrangements for wintering and for scientific observations, which will be of the greatest service to future travellers. In

connexion with the first, he introduced the improvement of carrying the cabin stairs not only down to the floor, but below this into the hold, and then bringing another stair from the latter up again into the antechamber of the cabin—an arrangement which was of extraordinary service in keeping up the temperature.

We come now to the last of the so-called Franklin-expeditions*. It was the fourth of the expeditions fitted out by Lady Franklin; and the command of it was entrusted to Captain M'Clintock. He sailed on the 1st of July 1857, from Aberdeen, in the screw-steamer 'Fox,' of 180 tons, with a crew in all of 25 men. The officers were Lieutenant Hobson of the Royal Navy as first, and the merchant-captain Allen Young as second officer. Besides these there were in the cabin a surgeon, Dr. Walker of Belfast, two engineers, and Petersen an interpreter. The stores consisted of 6000 pounds of pemmican and a large stock of preserved vegetables, with the well-known antiscorbutic remedies (lemon-juice and sugar), and was calculated for twenty-eight months.

The voyage was prosperous as far as Melville Bay; but when M'Clintock attempted to make his way into Lancaster Sound the ship got into pack-ice, became fixed, and drove with it down Baffin's Bay for 242 days. The first winter, therefore, had to be passed in the pack-ice; but the ice was quiet, and they were exposed to none of the perils which so frequently occur under similar circumstances. No cases of illness occurred; but the second engineer died in consequence of a fall in the engine-room. As soon as the ship was again set free, they turned once more towards the north, and succeeded this time in passing through Lancaster Sound. An attempt to sail down Franklin's (Peel's) Strait was unsuccessful, as it was completely blocked with ice in the narrow part. M'Clintock then attempted to push through Prince-Regent Inlet and Bellot's Strait into the southern part of Peel's Strait and so to King William's Land, but here also was prevented by ice from penetrating further.

Nothing then remained but to allow themselves to be frozen up in a small harbour in Bellot's Strait, and to do by sledge-journeys what could not be done with the ship. How far this was successful, how the greater part of the coasts of Peel's, Ross, and Victoria Straits was surveyed by M'Clintock, Hobson, and Young, and how Hobson found that important document which furnishes the only authentic intelligence of the condition of Franklin's expedition up to April 1848, is too well known to render it necessary for us to dwell upon it here.

* The Voyage of the 'Fox' in the Arctic Seas. London, 1859. And Petersen: Den sidste Franklin-Expedition med Fox, Capt. M'Clintock.

During this second winter the first engineer and the steward died—the former by an apoplectic attack, and the latter of scurvy, because he obstinately rejected all precautions, lived almost exclusively upon salt meat, and was also somewhat addicted to the use of spirits. Nearly all the crew suffered more or less from scurvy; and Lieutenant Hobson especially was rather severely attacked by it. However, all soon recovered. On the sledge-journeys, as might be expected, they were a good deal affected by frost; but all evil consequences disappeared on their return on board the ship. On the 23rd of September 1859 the ship lay in good condition in the docks of London.

The intelligence of the melancholy fate of Franklin's expedition was followed by the exhaustion natural after such enormous exertions. Since this period no arctic expedition has been sent out from England; but Dr. J. J. Hayes, the companion of Kane on his second voyage, procured the means of fitting out an expedition to Smith Sound*, and started well equipped from Boston, in July 1860, in the sailing schooner 'United States,' of 133 tons, with a company of fourteen men (among whom was the astronomer Sonntag, who had already accompanied Kane on his second voyage), to which were subsequently added three Europeans and three Eskimos, and, lastly, the Eskimo Hans with his family, already known by having accompanied Kane's expedition. His object was to reach a harbour on the east coast of Grinnell Land before the commencement of winter, and thence if possible to pass through Kennedy's Channel and penetrate into the polar sea seen by Morton. This object, however, he did not attain, but was obliged to remain in Port Foulke, 20′ of latitude further south than Rensselaer Harbour, and situated at the entrance of Smith Sound—much to his regret, as that sound is always difficult to pass through. From this point, where he took up his quarters for the winter, he attempted in October of the same year to make a sledge-journey into the interior of the country, but was compelled to return by a cutting north wind against which it was impossible to contend for any length of time. Nevertheless this short journey into the interior furnished interesting information as to the glaciers of Greenland.

Whilst the people specially fitted for them undertook scientific operations, such as meteorological, magnetic, and pendulum observations, the others were sent upon the chase, and brought an extraordinary quantity of game into the kitchen. As they had no dogs, Hayes sent Sonntag with Hans to the Eskimos living further to the south in order to procure some. After an absence of a month Hans returned alone, and reported that Sonntag had

* The Open Polar Sea, by Dr. J. J. Hayes. German edition by Costenoble, Jena, 1868.

fallen through a fissure of the ice into the water, had then gone several miles in his wet clothes, and died in a hut which they reached.

In the spring of the following year Hayes commenced one of the most toilsome sledge-journeys that has ever yet been made. Its object was to penetrate as far as possible upon the coast of Grinnell Land, and to reach the polar sea which had been seen by Morton. On the way he was obliged to leave behind him a portion of his party, and went forwards with only one young man of 19 years old (Knorr) and one dog-sledge, until his further progress was prevented, under 81° 35′ N. lat. and 70° 30′ W. long., by rotten ice and partially open water which extended as far as the eye could reach. He was compelled to abandon his desire of penetrating into this water with the ship, as Smith Sound was not free from ice this year; and so Hayes returned from his interesting voyage in the autumn of 1861, to Boston. The important scientific results of this expedition have been published by the Smithsonian Institution.

Conclusions.

With this the series of marine expeditions which have wintered in the north is for the present closed. They furnish evidence that with a little care a residence in the arctic regions is by no means impossible.

In the following Table the deaths which have occurred in these arctic expeditions, so far as we are able to find reliable statements, are summarized and their annual percentage for each expedition given, in calculating which the actual duration of the expedition has been taken into account, the duration of a voyage which extended over a single winter being reckoned as a year and one-third.

No.	Commander and year.	Ships.	Crews.	Deaths	Annual percentage.
1.	Parry, 1819–20	375 tons. 180 ,,	51 36	1	0·86
2.	John Ross, 1829–33	150 ,,	23	4	4·02
3.	Franklin, 1845–48	2 ships.	129	24	6·20
4.	J. C. Ross, 1848–49	1 ship.	138	7	3·80
5.	Saunders, 1849–50	1 ,,	40	4	7·50
6.	Austin, 1850–51	4 ships.	180	1	0·42
7.	Penny, 1850–51	2 ,,	46	0	0·00
8.	De Haven (Kane), 1850–51.	144 tons. 91 ,,	17 16	0	0·00
9.	M'Clure, 1850–54	1 ship.	66	5	1·75
10.	Belcher, 1852–54	2 ships.	90	2	0·95
11.	Kellett, 1852–54	2 ,,	90	4	1·91
12.	Pullen, 1852–54	1 ship.	40	0	0·00
13.	Kane, 1853–55	144 tons.	18	3	7·14
14.	M'Clintock, 1857–59	180 ,,	25	3	5·14
15.	Hayes, 1860–61	133 ,,	18	1	4·17
		Average...	....	....	2·92

Remarks.

1. Died of lung disease.

2. One of lung disease concealed on the voyage out; two of scurvy; one after return in consequence of hardships.

3. According to the information found by M'Clintock. Causes of death unknown; three died in the first winter (1845–46).

4. In consequence of a sledge-journey of forty days with insufficient provisions.

6. Sickly from the first; died in consequence of hardships on sledge-journeys.

8. Suffered much from scurvy; equipment rather hasty.

9. All the deaths in the last winter, from scurvy.

11. One of heart-disease; two from weakness in consequence of hardships; one on a sledge-journey.

13. Two died in consequence of a sledge-journey; one on the return voyage in consequence of a dislocation.

14. One in consequence of a fall; one of apoplexy; one of scurvy.

15. In consequence of a fall into the water. The Eskimos that Hayes had with him are omitted.

From this review it appears, therefore, that the percentage of deaths is on the average very favourable when compared with the mortality upon voyages in the tropics. The result would have come out much more favourably if we could have taken in Collinson's and some other winterings; but with regard to these reliable information was wanting.

Even when compared with the ordinary mortality at the age of 30, which, according to Milne's Carlisle Tables, amounts to 1·19 per cent., the result may be called very satisfactory, especially if we consider that most of the deaths occurred in consequence of great hardships upon sledge-journeys, or were produced either by diseases the germs of which were previously in existence, or by accidents which could not be foreseen; the last are possible upon any journey, even when it is not directed towards the North.

We believe that in the preceding statements we have furnished a proof that a winter residence in the arctic regions is by no means dangerous for Europeans, always supposing that the necessary precautions are taken. These precautions are as follows:—

First, a thoroughly good equipment of the ship, rendering its sides as strong as possible—partly to resist the pressure of ice, and partly for the sake of warmth. How the ship is to be prepared for its winter quarters we have already described circumstantially, and may therefore abstain from its repetition.

The second main point is good nourishing food, especially fresh meat and pemmican—salt meat being not good even for

the healthy, whilst for those affected with scurvy it is absolute poison. In order to keep off the latter disease, a certain and not too small quantity of lemon-juice and sugar must be taken daily, besides vegetables, the eating of which in abundance is of great benefit. Nowadays, when all these things can be so easily procured of good quality, there is not the least difficulty in provisioning a ship in the most suitable manner.

The third thing upon which the vigour and welfare of a wintering company depend is warm clothing, which should consist less of furs than of several layers of woollen stuffs one over the other.

XLII. *On a New Spectroscope, together with contributions to the Spectral Analysis of the Stars. By* F. Zöllner*.

IN recent times the spectrum-analysis of the stars, apart from its disclosures as to the physical constitution of the celestial bodies, has begun to claim attention in another and not less interesting direction; for it affords a prospect of demonstrating and, under favourable circumstances, even of measuring the influence which the component of the relative motion of the earth and of the star observed, acting along the line joining them, exerts upon the position of the lines of the spectrum in question.

A simple consideration shows that actions which two separated bodies exert upon one another through periodical impulses of finite velocity of propagation, must be modified by a steady alteration in the distance of the two bodies. To Doppler, in the year 1841†, is due the merit of having first recognized this influence, though the conclusions which he deduced therefrom as to the colour of the stars must be admitted to be incorrect, owing to his having neglected the invisible part of the spectrum.

The experiments of Ballot, Mach, and others have shown that, as regards sound, the influence in question is in accordance with the requirements of the theory.

In the case of light, it has not hitherto been possible to confirm by observations magnitudes of that influence which could with certainty be demonstrated; for even the cosmical motions, which are the greatest we can use for this purpose, are very small when compared with the velocity of the propagation of light.

Yet the great improvements which, since the discovery of

* Translated from Poggendorff's *Annalen*, September 1869, having been read before the Royal Saxon Society of Sciences, February 6, 1869.

† "Ueber das farbige Licht der Doppelsterne und einiger anderer Gestirne des Himmels," *Abhandlungen der Böhm. Ges. d. W.* vol. ii. (1841–42) pp. 465–482.

spectrum-analysis, have been made in the optical instruments for observing the spectrum, open out the prospect of demonstrating that influence on the spectra of the stars. Theory requires that this should consist of a small displacement of the spectrum-lines, which, for instance, for the mean velocity of the earth of 18·2 miles in a second, amounts to the tenth part of the distance between the two sodium-lines. This magnitude, which is very easily deduced from the velocity of light and the length of oscillation of the rays corresponding to the sodium-lines, has been quite recently again deduced by J. C. Maxwell in accordance with earlier calculations by F. Eisenlohr*.

Yet the magnitude of the displacement appears to Maxwell to be so small, that he concludes his observations with reference to the spectroscopes hitherto constructed and the method of determining the position of the lines with the remark, "it cannot be determined by spectroscopic observations with our present instruments, and it need not be considered in the discussion of our observations"†.

Huggins, nevertheless, in his most recent paper‡, of which the above-mentioned investigations of Maxwell form an integrant part, has attempted the solution of the problem in question by using a spectroscope with not less than five prisms, of which two are flint-glass Amici's, and three crown-glass.

The great enfeeblement of light produced by so great a number of prisms permits the observation of only the brightest stars. Huggins even restricts himself to the communication of his results from observations on Sirius, and thought he had here found a slight displacement of the line F compared with a bright hydrogen-line produced by a Geissler's tube. The direction and magnitude of the displacement would indicate an increase of the distance between the earth and Sirius with a velocity of 41·1 English miles in a second.

Eliminating the component of the earth's motion, which at the time of observation amounted to twelve miles, the velocity with which Sun and Sirius move apart would be 29·4 miles in a second.

Huggins himself considers this result as affected with great uncertainty—an uncertainty partly due to the enfeeblement of the light produced by numerous prisms, partly to the difficulty of comparing the coincidences of the *bright* lines of terrestrial luminous sources with the analogous dark ones of the star-spectra. The latter have at times a different appearance—are, for instance, indistinct at the edges and of variable breadth, as is just the case with this line F in the spectrum of Sirius.

* *Heidelberger Verh. d. phys. med. Ges.* vol. iii. p. 190.
† Phil. Trans. 1868, p. 532. ‡ Ibid. p. 535.

The most important of these difficulties which have heretofore hindered a definite solution of the problem in question, I think I have overcome by a new construction of the spectroscope, the first specimen of which I have the honour of laying before the Society.

The arrangement is essentially as follows:—The line of light produced by a slit or by a cylinder lens is in the focus of a lens which, as in all spectroscopes, first renders parallel the rays to be dispersed. The rays then pass through two Amici's direct-vision prisms, which I obtained of superior excellence from the optical workshop of M. Merz in Munich.

They are so fastened together that each of them transmits one-half of the rays emerging from the object-glass of the collimator, but so that the refracting edges are on opposite sides, and thus the total mass of rays is decomposed into two spectra of opposite directions. The object-glass of the observing-telescope, which again unites the rays to an image, is cut at right angles to the horizontal refracting edges of the prisms, as in the heliometer; and each of the two halves may be moved micrometrically, both parallel to the line of section and also at right angles thereto. Thus not only can the lines of one spectrum be successively made to coincide with those of the other, but both spectra, instead of being superposed, may be placed closed beside each other (so that one is displaced in reference to the other like a nonius), or they may be partially superposed. By this construction, not only is the delicacy of the double image as a means for determining any change in position of the spectrum-lines utilized, *but any such alteration is also doubled,* inasmuch as its influence in the two spectra is exerted in opposite directions.

The principle of the *reversion of the spectra*, fundamental to the instrument described (for which I therefore propose the name "*Reversion-Spectroscope*"), may be applied even without using Amici's systems of prisms. It is only necessary to reverse, by reflection from a mirror or from a prism, one part of the pencil of rays emerging from an ordinary prism, and then to observe the whole pencil as above by means of a telescope provided with a cut object-glass. This principle also dispenses with the simultaneous introduction of artificial sources of light for investigating small alterations of refrangibility, and enables those changes to be seen and measured by the alterations in position of *perfectly homogeneous objects*.

The series of measurements which were made with the dark lines D of the solar spectrum, as well as with the bright sodium-lines of the flame of a taper impregnated with salt, and which I here adduce as a criterion of the capability of the instrument, justify the hope that by means of this spectroscope

we shall succeed not only in detecting the influence of the earth's motion, but in determining its amount with such accuracy as is desirable for a preliminary control of theoretical conclusions.

The numbers adduced signify parts of the micrometer-screw, and refer to the distance of the two sodium-lines:—

	Sodium-flame.		Sun.
	49·5		49·5
	50·5		51·5
	53·0		48·1
	49·5		48·9
Mean . .	50·6 ± 0·6	Mean . .	49·6 ± 0·5

In the following series of observations the reversion-spectroscope had been provided with another micrometer-screw with a somewhat coarser thread, and also two other systems of prisms, the dispersion of which in the vicinity of the sodium-line is 1·77 as much as that of the system used for the above measurements. In this case, also, the former achromatic object-glasses of the collimator and of the observing-telescope were replaced by non-achromatic ones, whereby not only was there no loss of sharpness, but, as was intended, by increasing the intensity of light, there was a gain in clearness and distinctness.

Sun.

	Parts of the screw.	Deviations from the mean.
	67·1	−0·8
	69·4	+1·5
	68·4	+0·5
	67·9	0·0
	66·6	−1·3
	66·1	−1·8
	68·2	+0·3
	68·0	+0·1
	69·6	+1·7
Mean . .	67·9 ± 0·3	

Hence the distance of the two D lines would be determined with a probable error of $\frac{1}{226}$ of its magnitude. From what has been said above, an alteration of the distance between the source of light and the spectroscope with a velocity of nineteen miles in a second produces a reciprocal displacement of the lines of the two spectra amounting to one-fifth of that distance—a magnitude, therefore, forty times that above found as the probable error from the mean of nine readings.

Hence if, in observing stellar spectra, a sufficient quantity of light can be used, it may be definitely decided by the way de-

scribed whether the expected displacement of spectral lines occurs or not. In reference to the requisite intensity of light, I may be permitted to remark that I had a non-achromatic lens* of 1 Paris foot diameter and 6 feet focal distance; the pencil was received a few inches in front of its focus on a suitable concave meniscus of flint glass, and, thus freed as far as possible from spherical and chromatic aberration, it impinged on the slit of the spectroscope. I think I must here more especially point out that, in the use of a slit, achromatism of the optical image is not necessary for the observance of the spectrum, especially of individual parts of it, and that therefore the above construction may claim the advantage of being cheaper than when achromatic glasses of great luminous intensity have to be used. Of course in those cases in which the objects to be observed require as sharp separation as possible, as in the case of the double stars, this advantage must be given up.

I may be permitted, in conclusion, to make a few observations on problems and methods which refer to spectrum-observations of the sun, and with which I am at present occupied.

The sun possesses a velocity of rotation in virtue of which a point on its equator moves with a velocity of about a mile in a second. If, therefore, by means of a heliometer, or in any other way, a double image of the sun be produced, and if by suitable adjustment two points of the edge of the equator be brought into contact, parts of the sun's surface are bounded by the point of contact, of which one set move towards us and the other move from us with a velocity of the amount mentioned. There is thus produced a difference in the velocity of the parts touching of about two and a half miles. In accordance with what has been above said, such a magnitude of motion would produce an alteration in the position of the sodium-lines corresponding to the $\frac{1}{80}$th part of their distance. Hence if, by combining a sufficient number of prisms, such a magnitude can be perceived or measured, it is only necessary to bring the middle of the slit to the line of the two centres of the sun's pictures to see in the field of view of the spectroscope the two spectra of the sun's edges close to one another, and thus observe the displacement in question under the most favourable circumstances. In this manner the position of the sun's equator might be determined; and, provided the measurements could be executed, the velocity of rotation in various heliographic latitudes might also be determined, which would be of the greatest interest in reference to opinions recently expressed upon this subject.

Apart, however, from a quantitative determination of the phenomenon in question, by even a qualitative proof a simple means

* Constructed in the optical workshop of M. H. Schröder in Hamburg.

would be found *of separating all the lines which result from absorption in the earth's atmosphere from those which owe their origin to the sun's atmosphere,* inasmuch as the displacement in question can only affect the latter.

Another subject of investigation by spectrum-analysis of the sun are the protuberances. Lockyer and Janssen have, as is well known, succeeded in observing the spectrum of these objects (consisting of three bright lines) independently of a total solar eclipse.

At present attention is directed on all sides to finding out methods which shall enable not only those lines, but the entire figure of the protuberances to be simultaneously observed.

The position of the bright lines corresponds to the magnitude of the dimension of the protuberance in question which falls in the direction of the slit. When the slit is brought successively into various directions so that it cuts the protuberance in just so many positions, we are in a position to construct the shape of the body observed, as Lockyer has already done. Janssen has proposed the construction of a rotating spectroscope, so that, with adequate velocity of rotation, by means of the duration of the impression of light the shape of the entire protuberance might be seen at once.

Apart from the mechanical difficulties of such a rotating spectroscope, in which one of the three bright protuberance-lines must be exactly in the axis of rotation, the object in view might be more simply and completely obtained by oscillating the slit at right angles to its direction. We should then be in a position to observe the same protuberance simultaneously in three differently coloured images corresponding to the three different lines of its spectrum. Yet in these methods with a moveable slit, the difference in brightness, through which the protuberance stands out against the ground, is considerably enfeebled according to the distance traversed by the slit. With the rotating spectroscope more especially, the brightness of the protuberance would be weakened from the centre of rotation towards the edge, and the observation of the natural relative brightness of the image would be prevented.

For this reason I intend using another very simple means for attaining the object in question, of the practicability of which I have convinced myself by experiments (to be subsequently described) on terrestrial sources of light. The principles upon which this method depends are the following:—

(1) The apparent brightness (lustre, *claritas visa**) of a protuberance-band is independent of the breadth of the slit, provided that it always retains a perceptible breadth upon the retina.

* Lambert, *Photometria* &c. §§ 36 & 37.

(2) The brightness of the superposed spectrum increases proportionally to the breadth of the slit.

(3) With an oscillating or rotating slit the brightness of the superposed spectrum remains unaltered; that of the image of the protuberance decreases according to a law which depends upon the number and duration of the impressions produced on the place of the retina in question in the unit of time, and on the refrangibility of the observed protuberance-band.

Assuming, for simplicity's sake, that the entire surface over which the slit moved in its rotation or oscillation were occupied by the protuberance, and assuming that the intensity of the after-image formed were inversely proportional to that surface (corresponding to a uniform distribution over that surface of the light passing through the *stationary* slit), then assuming the above three principles, the ratio of the intensity between ground and protuberance would remain the same, whether,

First, by *oscillation* of the slit the brightness of the image of the protuberance were diminished, and thus the brightness of the superposed spectrum or of the ground (according to (2)) were left unchanged, or whether,

Secondly, the stationary slit was so far opened that its aperture just extended over the space over which in the first case the oscillation extended. Hereby, according to (1), the apparent brightness of the protuberance would remain unchanged, while that of the ground would be increased in the same ratio in which it was formerly weakened when the ground was unaltered.

Hence, under the above suppositions, the intended object would be far more simply attained in the second way, by taking care that, on account of dazzling, the intense direct light of the sun did not penetrate into the slit.

The slit need then only be opened so far that the protuberance, or a part of it, appears in the aperture. By polarizing or absorbing media, placed in front of the eyepiece, a suitable weakening of the entire field of view must be provided for, in order that the ratio between the intensities of the protuberance and superposed spectrum may be as striking as possible.

Led by these considerations, I have attempted to realize by means of terrestrial sources of light the conditions under which the protuberances are visible, in order thus to test both methods and convince myself of their practicability. In order the better to understand the experiments described, the following remarks may be premised.

The reason why, under ordinary circumstances, by deadening the intense solar image the protuberances are not visible at its edge, lies in the superposed strongly illuminated particles of our atmosphere. In a total solar eclipse this superposed light is so

considerably weakened, that then the intensely luminous protuberances stand out from the illuminated parts of the corona of the darkened sun. We may form an idea of the magnitude of the necessary enfeeblement of the diffuse light of our atmosphere, if we assume that the mean luminosity of the atmosphere during a total solar eclipse is equal to that during an average full moon. From my photometrical measurements* this luminosity is 618,000 times less than that produced by the sun. Hence the selective absorption of coloured media must stand in a similar ratio to that of the homogeneous light of the protuberance, if, as is attempted on various sides, we wished to make the protuberances visible without dispersion.

On the other hand, the possibility of attaining this object by the aid of the prism by dispersing the superposed atmospheric light depends essentially upon the circumstance that this light consists of rays of all refrangibilities, while that of the protuberances only consists of three homogeneous kinds of rays.

I have in the following manner produced artificially the superposition of a non-homogeneous mass of light over a body shining with homogeneous light and bounded by sharp outlines.

The wick of an alcohol-flame was impregnated with chloride of sodium and chloride of lithium. At a distance of eighteen feet from this flame, a piece of plate glass was so placed at an angle of 45° to the direction of observation, that the reflected image of a petroleum-flame at the side covered the feebly luminous alcohol-flame, and by its considerably greater intensity rendered it quite invisible. About a foot in front of the reflecting glass plate was a small lens of 6 inches focus, which threw an image of the alcohol-flame upon the slit of the spectroscope. The latter was fastened to the end of a spring about 10 inches long, by which, removed from its position of equilibrium and left to itself, it could for about five minutes be made to perform oscillations of sufficient amplitude.

The breadth of the slit was first of all so far diminished, that when the slit was at rest the double line D, and in comparison feebly the lithium-line, appeared well defined in the field.

When the slit was made to oscillate, these lines changed into sharp images of the alcohol-flame, of which the two soda images were about half covered. The apparent brightness of these three images was considerably smaller than that of the bright lines, and hence their prominence on the diffusely illuminated spectrum-ground smaller in the same ratio than that of the lines when the slit was at rest.

When now I applied the second of the above proposed methods, and opened the stationary slit so far that the image of

* *Photometrische Untersuchungen* &c. p. 105. Leipzig, 1865.

the alcohol-flame was just bounded by the rectangular slit, I was surprised by the far greater beauty and distinctness with which the images of the flame stood out from the diffusely illuminated spectrum-ground.

I may remark that I used in this experiment only *one* of the above-mentioned newer prisms; but it is clear that, with increasing dispersion, the enfeeblement of the superposed non-homogeneous light may be enhanced at pleasure.

In principle no difficulties prevent the application of this method to the sun's protuberances*. Yet practical success, with the given ratio of the intensities of homogeneous protuberance- and superposed atmospheric light, is essentially dependent on whether a sufficiently strong dispersion for this ratio can be attained. If, however, from the intensity and distinctness with which the lines of the protuberances appear, especially the middle one (of which I have convinced myself by my own observation at the Berlin Observatory on the 21th of last December), it is allowable to infer a very considerable relative brightness of the protuberances, the means now at my disposal (four excellent systems of prisms) will probably be sufficient to solve satisfactorily, in the way here proposed, the problem of the visibility of protuberances.

Leipzig, February 1869.

Appendix.

M. Faye, after giving an account to the Academy of Sciences, on September 20, of the above paper, proceeds as follows:—

"M. Zöllner has subsequently applied his new method to the sun with the most complete success. He has been able to follow and map from minute to minute with surprising facility and accuracy the magnificent phenomena of the chromosphere; he is even about to photograph them, utilizing the images due to the ray situated in the most photogenic part of the spectrum.

"Some of the drawings above mentioned have been published by Zöllner in a separate pamphlet. They show clearly that the protuberances are violent eruptions (Mr. Lockyer has already approximately determined their velocity), and not clouds suspended in an atmosphere. They might be said to consist of a gaseous mass projected vertically into an almost vacuous space, expanding almost immediately, and then falling more slowly, assuming the most capricious forms. Perhaps in this way we shall be able to group the new manifestations of the force which the sun exerts upon the very light material of comets,—a polar force, according to Bessel and Olbers, like electricity and magnetism; a force merely repulsive according to another hypothesis,

* Owing to my not having yet completely set up the necessary instruments, I have been unable actually to test this method.

with which M. Roche's beautiful researches are connected. In any case these drawings, which refer to four days, give the key to a very curious enigma presented by the eclipses observed in South America, in Chili, and in Brazil; I speak of the black protuberances. They seem to me to be due merely to the dark interval which exists for a few minutes either between two adjacent eruptions the plumes of which join, or between the ascending column of an eruption and its plume falling on the side of it.

"Thus to observe the protuberances with the spectroscope at any hour of the day, even when the sun is near the horizon, it is sufficient to open slightly the slit of the spectroscope. Perhaps M. Zöllner will succeed in seeing them all together as in an eclipse, by using very large prisms and a slit curved as an arc of a circle."

XLIII. *On the Structure of the Human Ear, and on the Mode in which it administers to the Perception of Sound. By* R. Moon, *M.A., Honorary Fellow of Queen's College, Cambridge.*

[Continued from p. 130.]

In my last paper I endeavoured to show:—

1. That the fact of the tympanal membrane being concave outwards, coupled with its flexibility, adapts it as an agent for the transmission to the sensorium of the motion arising from rarefied waves, while the same concavity, coupled with the inelastic and unyielding character of the membrane, forbids the transmission of the motion arising from condensed waves.

2. That if the ear yields to the impressions which rarefied waves tend to produce upon it, an apparatus will be required by means of which, after exposure to such waves, the membrana tympani may be brought back to its original position, and the organ generally be restored to its normal status; that the muscles acting upon the bones of the ear are calculated to perform that office; and that no other adequate function has ever been assigned to them; whence we may conclude that that portion of the auditory apparatus has been contrived with exclusive reference to the action upon the ear of rarefied waves.

3. That when either the tympanal membrane or the malleus or incus is wanting, or the latter of those bones is disconnected from the other or from the stapes, then, under the influence of rarefied waves, the oscillations between the vestibular and cochlear fenestræ of the fluid in the labyrinth will still be maintained by the alternate action, on the one hand of a difference in the external pressures upon the fenestræ, and on the other of the stapedius muscle; and that in this way a considerable power of

perception of sound may occur; at the same time, that when the ear is exposed to the action of condensed waves under the same circumstances no motion of the fluid in the labyrinth, and consequently no perception of sound can occur.

The question here naturally presents itself, if, when the membrana tympani, malleus, and incus are wanting, and the Eustachian tube ceases to perform any recognizable function, hearing occurs in a manner, in some cases, not very much less perfect than when the ear is in its normal state, how comes it that a machine so much more extensive and complicated is ordinarily resorted to by nature for the accomplishment of that object?

To this it has been replied, that in the perfect ear the machinery is much more efficiently protected from external injury, whether arising from foreign bodies which find their way into the meatus, or from cold*, than is the case with the mutilated organ.

It may be added, moreover, that, on the view of the auditory apparatus above set forth, the unyielding character of the tympanal membrane must operate to protect the organ from injury arising from condensations of the atmosphere, while the opposite actions of the tensor muscle and of rarefactions of air must tend to mitigate the effect upon the organ of the latter.

It may readily be conceived, too, in a general way, that the ear in its normal state must be a more powerful, more refined, and more manageable instrument than that presented by the simple labyrinth with its contents and closures, aided by the stapedius muscle only.

A more important consideration, however, still remains.

If we regard the importance and delicacy of the functions performed in the perfect ear by the two muscles combined and in the imperfect ear by the stapedius alone, if we consider that these muscles are under the influence of nerves which are not involuntary but are subject to the action of the will, if we advert to the very slow and gradual manner in which the recognition of articulate sounds is developed in infancy, if we take account of the apparently boundless interval between the capacity for appreciating sounds possessed by the obtuse rustic and by the finest musical genius—if we keep in view these various facts, I think it cannot but be evident that a long and most delicate process of education of the nerves and muscles must be passed through before that degree of proficiency is attained which is requisite for the comprehension of spoken language, and that one still more extended and refined must be undergone

* The inconvenience from this latter cause, when the membrana tympani is absent, is often very great. See papers by Sir Astley Cooper in the Philosophical Transactions for 1800 and 1801.

before reaching that degree of perfection with which many are capable of discriminating the most complicated harmonies.

This process of education may be surmised to be greatly facilitated by the possession of the complete and perfect instrument; and it by no means follows that, because the education once acquired through its instrumentality can to a certain limited extent be turned to account by the imperfect organ, therefore the needful training could equally have been attained by the aid of the latter alone.

The relation of the ear in its normal condition to the ear deprived of the membrana tympani may be likened to that between a violin with the ordinary provision of four strings and the same instrument when three of its strings have been taken away: with regard to which it may be observed that, although in the latter case a musical prodigy has been known to elicit from it effects which, in the absence of actual experience, would have passed belief, it is at the same time clear that, without the skill and dexterity acquired upon the more perfect instrument, no such effects could have been producible.

I now propose to advert to one or two miscellaneous points of interest connected with the subject.

I. I would in the first place recall attention to the description of the muscles of the ear already cited from Mr. Wharton Jones (vide *antè*, p. 125), who informs us that the muscles attached to the malleus have been by some anatomists [herein following Sömmerring] stated to be three in number, of which two are laxative and one a tensor of the tympanal membrane. Of these Mr. Jones declares that the last named only can be strictly demonstrated, and that the supposed laxatores tympani are simply ligaments.

Now of these latter it is clear that, had they been attached to muscles which would have relaxed the tympanum, being of the nature of tendons and therefore fibrous and inextensible, they would operate to resist any further stretching of the membrana tympani; so that if the membrane had been elastic (which, as has been shown, and as is well known, it is not), and to that extent capable of being stretched by the action of condensed waves incident upon it, these so-called *laxatores tympani* would prevent any such effect taking place, and would thus, as it would appear, have been of themselves sufficient to obviate any action upon the sensorium of condensed waves—thus showing that the laxatores tympani ligaments tend to corroborate the effect resulting from the inelastic character of the membrane.

II. The foregoing conclusion is of peculiar importance when we come to consider the auditory apparatus of birds, in which and in that of mammalia alone is to be found a true tympanum.

The apparatus among mammalia is essentially the same in character as in man. That of birds differs (so far as regards our present purpose) in two features:—first, that the bones are in part replaced by cartilage, and, as regards their mutual collocation, are somewhat differently arranged; secondly, that the tympanal membrane is convex outwards, and not concave outwards as in the case of mammalia.

The apparatus in birds may be described as consisting of the labyrinth and of a single true bone (which from the correspondence of its functions with those of the stapes in mammals may be designated as a stapedal bone), connected with the upper part of which and with the sides of the tympanal cavity is a cartilaginous appendage to which the tympanal membrane is attached, and by which the membrane is supported in its convex (outwards) position as upon a bent spring.

A reference to the principles unfolded in my former paper will make it evident that the membrana tympani being convex outwards, its want of elasticity (even if it were inelastic) would oppose no obstacle to the transmission to the sensorium of the action of condensed waves; so that, so far as this part of the apparatus is concerned (whatever may be the case in man and in mammals), birds might have perception of sound through the agency of waves of condensation—an instrument of conveyance which, as has been stated, is slower, and therefore less efficient than is offered by waves of rarefaction.

Any such effect as that just described is obviated by means of a fibrous band stretching from the neighbourhood of the Eustachian tube, and attached at its other extremity to the cartilaginous appendage before spoken of; which band, for the purpose we are now considering, may be regarded as replacing the laxatores tympani in man and in mammalia. Respecting this band, M. Breschet informs us that "Lorsqu'on la tiraille, on opère la tension de la membrane du tympan"*; that is, the effect of the band, if it were attached to a muscle (which it is not), would be, when the muscle was contracted, to *draw* the tympanal membrane *outwards*; and its effect in the (actual) absence of any muscle attached to it must be *to resist any tendency to force the membrane inwards*; that is, its effect is to counteract the only effect capable of being exerted upon the membrane by a condensed wave.

III. Having shown the manner in which the auditory apparatus in birds is adapted to suppress the action upon it of condensed waves, it may be proper to point out the mode in which rarefied waves operate upon it.

* *Recherches Anatomiques et Physiques sur l'Organe de l'Audition chez les Oiseaux* (Paris, 1836), p. 24.

The tympanum of birds is provided with a single muscle only, the effect of which, when contracted, is to *relax* the membrane, *i. e.* to draw it inwards (Breschet, pp. 24, 30). Hence the position of equilibrium of the auditory apparatus of birds (*i. e.* the position which it assumes when not acted upon by any sound) may be defined to be that in which it is placed when the muscle or muscular fibres connected with the organ have produced their utmost effect, by drawing in the membrana tympani to the full extent which the fibrous band above mentioned will admit of; in which position, of course, the membrane will be incapable of being forced further inwards through the action of condensed waves.

If a rarefied wave be incident upon the organ when in this position, the tendency would be of course to move the tympanal membrane *outwards*; and the membrane being convex outwards, in order that such motion outwards may occur one of two things must happen—namely, either the membrane must be elastic, or else it must, when in the position of equilibrium, be somewhat loosely stretched upon the cartilaginous spring of which we have spoken.

I have nowhere found any statement as to the elasticity or inelasticity of the tympanal membrane of birds; but for the sake of perspicuity I shall assume, as seems most probable, that, like the tympanal membrane in mammalia, it is inelastic, and consequently that in the position of equilibrium the membrane rests loosely on the cartilaginous spring which supports it.

When the general apparatus is in equilibrium, we may suppose that the cartilaginous spring which forms part of it will also be in equilibrium. But when through the action of a rarefied wave the membrane has been moved outwards, the elasticity of the spring will immediately come into play, and will tend to bring the membrane back to its original position—a contrast being presented in this respect in the apparatus in birds and in mammalia; for whereas in the latter case, when the membrane has been moved outwards, the muscles of the tympanum are the essential and only means of bringing back the organ to its original status, there are in the former case two different and efficient agents for producing the same result—to wit, the elasticity of the cartilaginous spring and the tympanal muscle. It may be observed, however, that although the elasticity of the spring would in the first instance tend to bring back the membrane in the manner above described, there can be no doubt that, when the membrane had reached the position in which its further motion inwards would be stopped by the fibrous band above spoken of, it would receive a sudden and complete check; and this occurring at a time when its velocity was a maximum, the membrane would

rebound and again move outwards. A single atmospheric pulse might thus throw the auditory apparatus into a state of oscillation for a considerable time—a circumstance which would materially interfere with the distinct perception of articulate sound. To obviate such an effect is the special function of the tympanal muscle in birds.

It is worthy of remark that, although in the auditory apparatus of birds recourse is had to the principle of elasticity to the extent above explained, the principle requires to be kept in check, and is kept in check in the manner above described. In the more perfect organ of man and of mammals, on the other hand, the uncertain and unmanageable principle of elasticity is throughout excluded, the tympanal membrane, the ligamento-fibrous membrane wrapped about the base of the stapes, and the membrane of the fenestra rotunda being alike inelastic and inextensible.

IV. I would next remark that the success of the experiment of Valsalva (which, though in general only temporary in its effects, I apprehend to be of all known means for the diminution of deafness the most simple and the most universal of application) is confirmatory of the views with regard to the mode of action of the human ear which I have endeavoured to set forth.

For if, as I have stated, the sensation of hearing is produced primarily by the tympanal membrane and the stapes being forced outwards, and the cochlear membrane being drawn inwards by the operation of rarefied waves, and secondarily by these parts of the apparatus being restored to their former status through the operation of the muscles of the ear, the first and most natural step to be taken in any case of defective hearing is obviously to strengthen the tendency to move outwards of the tympanal membrane and stapes when under the influence of rarefied waves; and this will clearly be effected by Valsalva's experiment*, by which the density of the air in the tympanal cavity is temporarily increased. The enhanced effect of the experiment, as performed under the improved method introduced by Politzer, is thus also strikingly accounted for.

In the cases to which it is applicable (that is, when the tympanal membrane is wholly or in part present, and the connexion between the ossicles is wholly or partially maintained) the effect of Valsalva's experiment, upon the principles before explained, is precisely that of raising the voice in speaking to the patient.

On the other hand, if hearing took place through the agency of condensed waves, the result of the experiment would be to diminish the difference of the pressures on the two sides of the

* By this experiment, the nose and mouth being closed, air is forced through the Eustachian tube into the tympanal cavity.

tympanal membrane. If this assumption were true, therefore, Valsalva's experiment would occasion deafness rather than remove it.

V. As a particular instance under the last head, we may take the case where the tympanal membrane is relaxed.

The deafness hence arising is known to be temporarily relieved by Valsalva's experiment; and that it is so may be explained in this way:—When a rarefied wave is incident, its effect will be immediately to move the tympanal membrane outwards; but, on account of the relaxed state of the membrane, the effect will not be immediately to move out the stapes, the moving out of which is essential to produce the sensation of sound. Before this latter effect can be produced the membrane must be moved outwards until it becomes tightly stretched; and when it is so stretched, and not till then, the stapes will begin to move outwards. We may thus see how relaxation of the membrane diminishes the hearing-power.

VI. In contrast with the foregoing may be taken the following case related by Menière*:—"An old judge had been accustomed for at least sixteen years, by pressure of a blunt gold needle against the membrana tympani, to make himself, for an hour or so, a tolerably good hearing-power. Menière examined the ear during this state of things, found the membrana tympani uninjured, and that the pressure was made upon the handle of the malleus, which was pressed somewhat inwards. He speaks of having seen several similar cases, and considers them cases of nervous deafness, which were improved to a certain degree by pressure upon the ossicula auditus, and through them on the labyrinth."

I think there can be no doubt that the explanation here suggested (if it can be called such) is erroneous. In elucidation of the case before us, I give the following passage from Dr. Brennan's article on Elasticity, in the Cyclopædia of Anatomy and Surgery †:—

"When the disturbing force .... is slowly applied, there appears to exist some degree of elasticity, even in fibrous membranes; thus in *hydrops articuli* the structures about the joint are frequently much distended by the accumulation of fluid within, upon the absorption of which they slowly resume their proper condition."

The true explanation of the case in Menière I take to be, that in the undisturbed state of the patient's ear, before the application of the needle, the tympanal membrane was unnaturally tight-

* The citation which follows in the text is taken from an American translation of Von Tröltsch's Lectures, Philadelphia, 1864.

† The passage here cited immediately follows the statement as to the inelastic character of fibrous membrane quoted in my former paper.

ened in such a manner as to draw out the stapes, whereby the auditory apparatus, before the sonorous impressions became incident upon it, was placed in a state unfavourable for their reception. By the action of the needle the tympanal membrane would become stretched, thus allowing the stapes to assume its proper position; and this effect would continue until, by the gradual but slow recovery by the membrane of its former status, in the manner described by Dr. Brennan, the original obstacle to the hearing of the patient would recur.

VII. In conformity with the views which I have endeavoured to explain, loud sounds may be expected to produce deafness either (1) by rupture of the tympanal membrane, (2) by disconnexion of the chain of ossicles either from one another or from the tympanal membrane, or (3) by sudden convulsive action of the muscles of the tympanum, through which the stapes becomes so firmly fixed in the fenestra ovalis as to be with difficulty withdrawn.

I conceive that deafness might result, in the manner last mentioned, even in cases where the sound which is the cause of it is not exceptionally loud, provided that it was so sudden and unexpected as to cause alarm.

Probably also there is a fourth mode in which, in the case of loud sounds, deafness might result, namely where a great concussion of the air occurs; in which case the tympanal membrane may become stretched by reason of the unusual pressure exerted upon it by the *condensed* wave, in the manner in which Dr. Brennan describes it as capable of being stretched by the continued action of a more moderate pressure. The same cause which stretched the membrana tympani would force in the stapes, and thus tend to produce the same kind of deafness as No. 3 just referred to.

VIII. The mode in which deafness is sometimes relieved by means of a loud sound falling upon the ear is readily explicable upon the principles before set forth, if we suppose the deafness to have resulted from the stapes having become too firmly imbedded in the fenestra ovalis, or from rigidity of the articulations of the ossicles.

IX. In accordance with the same principles, nervous deafness may be expected to occur in either of two ways, viz. by paralysis or torpor (1) of the auditory nerve proper, (2) of the motor nerves connected with the muscles of the tympanum.

I shall seek for another opportunity to point out the functions of the membranous labyrinth and the semicircular canals*.

6 New Square, Lincoln's Inn,
October 1, 1869.

* In connexion with the explanation given in my former paper of the

XLIV. *Theory of the Voltaic Pile.* *By* W. Kencely Bridgman, *L.D.S.**

THERE are extant at the present time two theories of the voltaic pile, neither of which, however, can be said to be sufficiently satisfactory to set the matter altogether at rest.

The conclusions arrived at by the late Professor Faraday were to the effect that the source of power in the battery was derived from "the chemical force alone" (Experimental Researches, 2053); but as chemical force is not supposed to be able to originate itself, or to become developed otherwise than by generation from some antecedent force or forces, the disturbing cause, or initiating step whereby it becomes excited to action, still remains for elucidation.

On the other hand, Professor Tyndall expresses his belief in "the contact electricity of Volta being a reality," *though it could produce no current,* and goes on to observe that Sir William Thomson "and others now hold what may be called *a* contact theory, which, while it takes into account the action of the metals, also embraces the chemical phenomena of the circuit" (Faraday as a discoverer, by John Tyndall, note, p. 66); but as Faraday has demonstrated in the clearest possible manner (Exp. Res. 879–883) that metallic contact is not requisite for the completion of the circuit and obtaining the current, it can scarcely be admissible to recognize contact of the metals as one of the conditions necessary to the action of the battery.

In conducting the Experimental Researches relating to the action of the battery, Faraday starts with the assumption that "when an amalgamated zinc plate is dipped into dilute sulphuric acid, the force of chemical affinity exerted between the metal and

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action of the auditory apparatus when the tympanal membrane is absent, I may mention that I am assured by an eminent aurist that when the membrane is absent the interposition of the promontory would prevent the exposure of the cochlear membrane to the direct action of a wave of sound which had traversed the meatus externus, and that the latter membrane could only be reached by a reflected wave.

I may observe that the statement (p. 126, note) as to the action of the stapedal muscle, so far as the tympanal membrane is concerned, is perhaps made too positively. Whatever that action may be, I apprehend that it must always be subordinate to the action of the tensor tympani; so that while the joint effect of the two muscles combined must necessarily be to draw in the membrana tympani, that of the smaller and weaker muscle may be to effect some minute adjustment of the form of the membrane. A similar remark would apply to the functions of the laxatores tympani muscles, if upon further examination it should appear that such muscles exist.

* Communicated by the Author.

the fluid is not sufficiently powerful to cause sensible action at the surfaces in contact and occasion the decomposition of water by the oxidation of the metal" (Exp. Res. 893).

Again, in reference to a cylinder of amalgamated zinc placed inside a double cylinder of copper, and the two then inserted within a jar of dilute sulphuric acid, it is asserted that "being thus arranged there was no chemical action whilst the plates were not connected" (957); and "a battery constructed with the zinc so prepared (that is, *amalgamated*), and charged with dilute sulphuric acid, is active only whilst the electrodes are connected, and ceases to act or be acted upon by the acid the instant the communication is broken" (1000).

The very decided manner in which the assertion, *that no chemical action takes place unless the dissimilar metals of the battery be put into communication*, is made, and the frequency with which the belief in it is reiterated in various forms, make it appear that this supposed fact was considered of some importance in connexion with the conclusions arrived at. If, however, it be put to the test of examination, it will be found to receive a direct negative from experimental evidence and shown to be altogether a fallacy.

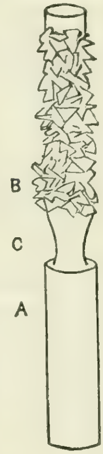
A rod of absolutely pure zinc, $3\frac{1}{4}$ inches long and weighing 487 grains, after being thoroughly amalgamated and drained, was placed half its length in cold dilute sulphuric acid (one part pure acid to ten of water), and the other half exposed to the atmosphere in the same position as the ordinary plates of a battery. In a very short time bubbles of hydrogen made their appearance over the whole of the surface exposed to the acid, and after forty-eight hours the zinc was found to have lost upwards of two grains in weight. This loss, however, was by far the least important part of the results obtained. The immersed portion of the metal had not been acted upon uniformly over its entire surface, but the action had been greatest at the surface of the liquid; at the same time the exposed portion had become covered with patches of crystalline sulphate of zinc, high and dry upon the projecting part of the metal. In addition to the fact of chemical action having been exerted between the metal and the acid and the water decomposed, there is the further evidence of the metal *having been polarized.*

In order to render the effect more apparent, the experiment was repeated with copper instead of amalgamated zinc, as the colour of the crystals and the colouring of the acid afford more conspicuous evidence of the results which are being produced.

A piece of stout copper wire was then similarly placed in acid; the latter very soon gave signs, by the colouring it received,

of the former commencing to undergo solution; and after having been suffered to remain undisturbed for twenty days, it presented the appearance exhibited in the diagram, fig. 1.

Fig. 1.



The portion A which had been immersed in the acid was partially corroded into pits and furrows, gradually decreasing in extent downwards.

The upper end, B, exposed to the atmosphere had become coated with a layer of minute and beautiful crystals of sulphate of copper, extending from the top down to within about three-sixteenths of an inch of the liquid.

At the intermediate portion, C, a greater amount of chemical action had been induced—corroding the wire, as represented, about halfway through and forming a neck tapering upwards.

The solution containing the end A was only slightly tinged in proportion to the amount of copper dissolved, the crystallization having been derived almost wholly from the metal above the surface of the liquid.

"It is at present generally admitted that, in the normal condition, the atmosphere is charged with positive electricity . . . . The terrestrial globe, on the contrary, is charged with negative electricity, as is proved by a variety of observations, direct and indirect; it is, moreover, a consequence of the presence of positive electricity in the atmosphere; for one of the electricities cannot manifest itself in the free state without the appearance of an equal quantity of the other kind"*.

It is a fair inference to assume that it is in obedience to this law that the exposed portion of the metal has been rendered electro-*negative,* as its behaviour indicates it to be, while that submitted to the acid has taken the opposite or electro-*positive* state.

That the action which arises between the metal and the acid is due to polarization is evidenced by the following proceeding.

A piece of copper wire *wholly submerged* in the acid so as to entirely exclude any portion of it from coming into contact with the air, has remained for many months without imparting the slightest tinge to the liquid. Another portion having a piece of platinum-foil connected with it has been attended with similar results. A piece of *un*amalgamated zinc-foil has also been kept in dilute acetic acid in the same way with equal effect.

But on suffering the liquid to evaporate so as to bring the

* Phil. Mag. S. 4. vol. xxxiv. p. 322, "Note on the Electrical Condition of the Terrestrial Globe," by A. De la Rive.

upper end of the metal near to its surface, the instant the slightest portion becomes exposed chemical action immediately begins.

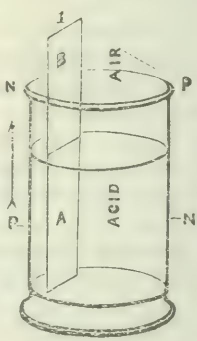
The first perceptible indication of this polarization is in the partial dewing of the copper immediately above the surface of the liquid. This gradually increases in extent until the whole exposed portion becomes wet with the solution, after which minute crystals soon make their appearance and in time cover the exposed part, as shown in fig. 1. The determination of fluids to the negative portion causes the acid to rise and spread itself over the surface of the metal; and this, becoming saturated in its ascent, furnishes the material from which the crystallization is derived.

Two equal portions of wire were similarly placed in acid, only that one was fully exposed to the atmosphere in an open tube, while the other was placed in a phial, the acid occupying half its height, and was kept closely corked for several weeks—after which the fully exposed metal had lost in weight two-fifths more than the one which had been excluded from contact with fresh portions of air, showing that contact with the atmosphere in bulk is necessary to the fullest action.

A piece of copper wire 3 inches long was immersed one-third in dilute *acetic* acid and exposed to the atmosphere in an open tube. In a very short time a dull coating of amorphous acetate of copper had been formed on the surface as far as the vapour of the acid had reached; but by degrees this dry formation became moistened, and as this occurred it was at once converted into minute and beautiful dark-green crystals.

In each of these instances it is thus indisputably shown that, in the position in which the plates of the battery are placed (that is, one portion immersed in the exciting liquid and the other exposed to the air), *chemical action does invariably occur, and is in fact an inevitable consequence of such partial immersion*; and taking place where there is no sufficient normal affinity existing between the metal and the acid to effect the decomposition of water, but arising from the metal being first polarized by the atmosphere, there is hence an additional element introduced that assumes a very significant character when applied to the composition of the battery.

Fig. 2.



Let A B, fig. 2, represent the zinc element of the battery immersed half its length in the acid. The condition it immediately assumes will correspond to that shown in fig. 1—that is, the upper end *negative*, and the immersed end *positive*.

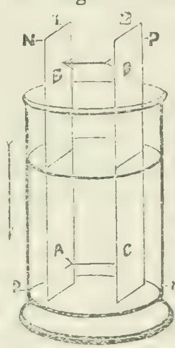
It will now appear that there are two

pairs of poles, namely, the metal B and the air above, and the metal A and the acid below, or a voltaic series composed of one metal and two fluids.

But as the air is a non-conductor, no current can yet be obtained. It is essential therefore to insert a conductor as its representative which shall retain the same relative condition of polarity, this polar condition being secured by its having a less affinity for oxygen than the zinc or primary metal.

A secondary plate of platinum, as in fig. 3, being substituted for the acid and the air of fig. 2, gives an arrangement of two equally polarized plates with their alternate poles in opposition; and having their lower poles joined by a conducting medium, they require only to be connected by their upper poles or electrodes to complete the circuit.

Fig. 3.



While separate, the chemical action is confined to the primary plate, and takes place in an upward direction; but immediately the electrodes are put into communication with each other, the action is diverted to the negative opposed to it in the conducting acid, and is now spread uniformly over the whole surface of the immersed metal. The polarization of the electrodes is thus shown to constitute an integral part of the battery itself; and these, by the addition of conducting-wires, are only made to undergo an extension of surface without alteration of electrical condition.

It is now obvious that placing between the electrodes any conducting substance capable of being decomposed must effect a corresponding action to that which takes place in the exciting fluid, and that an equal amount of chemical action will be effected at either end of the metals. Metallic contact, however, will reduce the two pairs of poles to one, as in the case of the horseshoe magnet, and thus effect a concentrated action.

In the first instance the secondary platinum plate only represents the polarity of the acid and the atmosphere; but on immersing the primary plate, and on this becoming equally polarized and combining with the oxygen of the electrolyte, there is a definite amount of hydrogen liberated, which retains its combining force unbalanced, and which then augments the charge of the secondary plate in an equal degree, and thus imparts to it a feeble degree of tension additional to the first power of the combination.

The chemical action occurring with the single metal chiefly at the surface of the fluid and but feebly within the acid lower

down, exerts only a trifling amount of force upon the secondary metal; but the instant the connexion is made through the electrodes, the whole of the electrolyte enclosed between the metal poles becomes electrolyzed and its *ions* separated, increasing the electromotive force in like proportion.

The contact of two dissimilar metals in air does *not* represent the two dissimilar metals of the battery, but simply corresponds with the two electric states of the primary metal alone. Scarcely any two metals have an equal affinity for oxygen, and any two of these placed together at once become polar and determine the mixed gases of the atmosphere to their respective poles. The combination which then takes place between the more oxidizable metal and the oxygen evolves or induces a certain amount of electrical force by which the combined metals and the adjacent portions of air become charged respectively positive and negative.

In the chemical action which takes place with the polarized primary alone, it was stated that the greatest amount of chemical action was found to occur near to the surfaces of air and acid in contact. The determination of oxygen from the atmosphere to the positive metal, combined with the electrolysis of the electrolyte, was here exhibited in the greater extent of oxidation and solution of the metal, and the less degree exhibited in the metal which had been partly excluded from the atmosphere.

That no current can be obtained from the contact of two metals in air is due to the fact that the atmosphere is not an electrolyte. It was distinctly defined by Faraday that no current is obtainable from chemical action unless by the decomposition of an electrolyte, the *cation* from which being absolutely indispensable for creating the tension of the secondary metal. The oxygen of the air having no *cation* to part with, is therefore unprovided with the means of accomplishing it.

The fact of this non-combination of the elements of the atmosphere constitutes the means of initiating the action of the battery. The electrolyte of the battery being held together by a combining force, cannot of its own accord separate itself into its component elements, but requires the introduction of some antagonistic force equivalent to or counterbalancing its cohesion, so as to set its elements free—to repolarize them in fact; this is accomplished by the introduction of the polarized metal, which, rendering the force equal on all sides, *electrolyzes* the water and allows its elements to rearrange themselves according to the polar influences then presented to them.

Were the atmosphere an electrolyte, it would then require some antecedent to effect *its* electrolysis, as the action must begin by a non-combination of elements, or a condition requiring no antecedent.

Norwich, September 1869.

XLV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 320.]

May 27, 1869.—Lieut.-General Sabine, President, in the Chair.

THE following communications were read:—

"Researches on Turacine, an Animal Pigment containing Copper." By A. W. Church, M.A. Oxon., Professor of Chemistry in the Royal Agricultural College, Cirencester.

From four species of *Touraco*, or Plantain-eater, the author has extracted a remarkable red pigment. It occurs in about fifteen of the primary and secondary pinion-feathers of the birds in question, and may be extracted by a dilute alkaline solution, and reprecipitated without change by an acid. It is distinguished from all other natural pigments yet isolated, by the presence of 5·9 per cent. of copper, which cannot be removed without the destruction of the colouring-matter itself. The author proposes the name *turacine* for this pigment. The spectrum of turacine shows two black absorption-bands, similar to those of scarlet cruorine; turacine, however, differs from cruorine in many particulars. It exhibits great constancy of composition, even when derived from different genera and species of Plantain-eater—as, for example, the *Musophaga violacea*, the *Corythaix albo-cristata*, and the *C. porphyreolopha*.

"On a New Arrangement of Binocular Spectrum-Microscope." By William Crookes, F.R.S. &c.

The spectrum-microscope, as usually made, possesses several disadvantages: it is only adapted for one eye*; the prisms having to be introduced over the eyepiece renders it necessary to remove the eye from the instrument, and alter the adjustment, before passing from the ordinary view of an object to that of its spectrum and *vice versâ*; the field of view is limited, and the dispersion comparatively small.

I have devised, and for some time past have been working with, an instrument in which the above objections are obviated, although at the same time certain minor advantages possessed by the ordinary instrument, such as convenience of examining the light reflected from an object, and comparing its spectrum with a standard spectrum, are not so readily associated with the present form of arrangement.

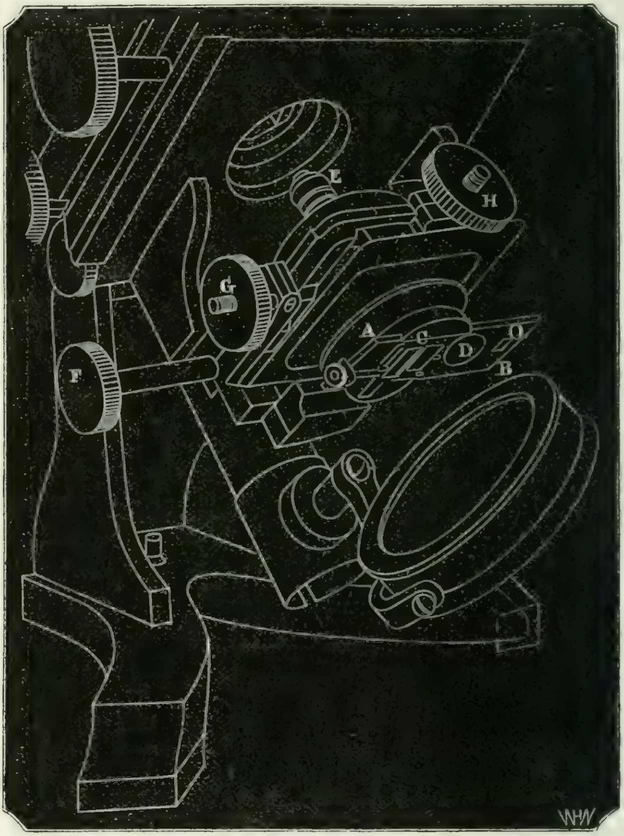
The new spectrum-apparatus consists of two parts, which are readily attached to an ordinary single or binocular microscope; and when attached they can be thrown in or out of adjustment by a touch of the finger, and may readily be used in conjunction with the polariscope or dichrooscope; object-glasses of high or low power can be used, although the appearances are more striking with a power of

* Mr. Sorby in several of his papers (Proc. Roy. Soc. 1867, xv. p. 433; 'How to Work with the Microscope,' by L. Beale, F.R.S., 4th edition, p. 219) refers to a binocular spectrum-microscope; but he gives no description of it, and in one part says that it is not suited for the examination of any substance less than $\frac{1}{10}$ of an inch in diameter.

$\frac{1}{2}$-inch focus or longer; and an object as small as a single corpuscle of blood can be examined and its spectrum observed.

The two additions to the microscope consist of the substage with slit &c., and the prisms in their box. The substage is of the ordinary construction, with screw adjustment for centring, and rackwork for bringing it nearer to or withdrawing it from the stage. Its general appearance is shown in fig. 1, which represents it in position. A B is a plate of brass, sliding in grooves attached to the lower part

Fig. 1.



of the substage; it carries an adjustable slit, C, a circular aperture, D, 0·6 inch in diameter, and an aperture, O, $\frac{1}{8}$ inch square. A spring top enables either the slit or one of the apertures to be brought into the centre of the field without moving the eye from the eyepiece. Screw adjustments enable the slit to be widened or narrowed at will, and also varied in length. At the upper part of the substage is a

screw of the standard size, into which an object-glass of high power is fitted. E represents one in position. I generally prefer a $\frac{1}{2}$-inch power; but it may sometimes be found advisable to use other powers here. The slit C and the object glass E are about 2 inches apart; and if light is reflected by means of the mirror along the axis of the instrument, it is evident that the object-glass E will form a small image of the slit C, about 0·3 inch in front of it. The milled head F moves the whole substage up or down the axis of the microscope, whilst the screws G and H, at right angles to each other, will bring the image of the slit into any desired part of the field. If the slide A B is pushed in so as to bring the circular aperture D in the centre, the substage arrangement then becomes similar to the old form of achromatic condenser. Beneath the slit C is an arrangement for holding an object, in case its surface is too irregular, or substance too dense, to enable its spectrum to be properly viewed in the ordinary way*.

Supposing an object is on the upper stage of the microscope (shown in fig. 2) and viewed by light transmitted from the mirror through the large aperture D and the condenser E, by pushing in the slide A B so as to bring the slit C into the field, and then turning the milled head F, it is evident that a luminous image of the slit C can be projected on to the object; and by proper adjustment of the focus, the object and the slit can be seen together equally sharp. Also, since the whole of the light which illuminated the object has been cut off, except that portion which passes through the slit, all that is now visible in the instrument is a narrow luminous line, in which is to be seen just so much of the object as falls within the space this line covers. By altering the slit-adjustments the length or width of the luminous line can be varied, whilst, by means of the rackwork attached to the upper stage, any part of the object may be superposed on the luminous line. The stage is supplied with a concentric movement, which permits the object to be rotated whilst in the field of view, so as to allow the image of the slit to fall on it in any direction. During this examination a touch with the finger will at any time bring the square aperture O, or the circular aperture D into the field, instead of the slit, so as to enable the observer to see the whole of the object; and in the same manner the slit can as easily be again brought into the field.

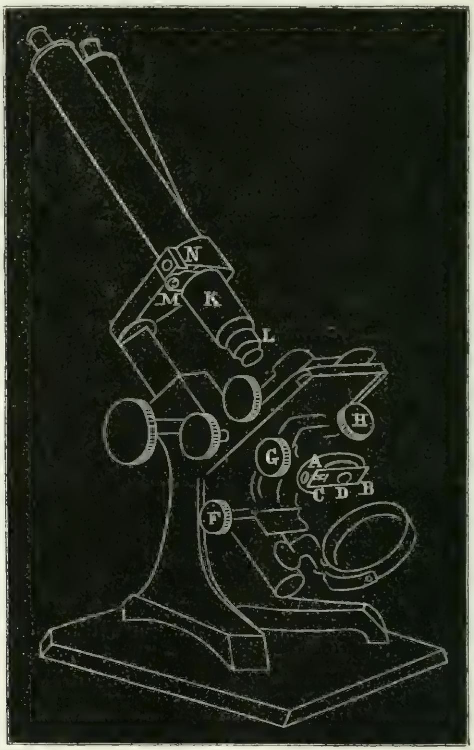
The other essential part of this spectrum-microscope consists of the prisms. These are enclosed in a box, shown at K (fig. 2). The prisms are of the direct-vision kind, consisting of three flint and two crown, and are altogether 1·6 inch long. The box screws into the end of the microscope-body at the place usually occupied by the object-glass; and the object-glass is attached by a screw in front of the prism-box. It is shown in its place at L. The prism-box is suffi-

* In carrying out the experiments which were necessary before this spectrum-microscope could be made in its present complete form, I have been greatly assisted by Mr. C. Collins, Philosophical-Instrument Maker, 77 Great Tichfield Street, to whom I am also indebted for useful suggestions as to the most convenient arrangement of the different parts, so as to render them easily adapted to microscopes of ordinary construction.

ciently wide to admit of the prisms being pushed to the side when not wanted, so as to allow the light, after passing through the object-glass, to pass freely up the tube K. A pin at M enables the prisms to be thrown either in or out of action by a movement of the finger.

As the prisms are close above the object-glass, the usual sliding box, carrying the binocular prism and the Nicol's prism (shown at N), may be employed as usual, and the spectrum of any substance may thus be examined by both eyes simultaneously, either by ordinary light, or when it is under the influence of polarized light. The insertion of the prism-box between the object-glass and the body of the microscope does not interfere with the working of the instrument in the ordinary manner. The length of the tube is increased 1 or 2 inches, and a little additional rackwork may in some instruments be necessary when using object-glasses of low power. The stereoscopic effect when the Wenham prism is put into action does not appear to be interfered with.

Fig. 2.



For ordinary work both these additions may be kept attached to the microscope, the prisms being pushed to the side of the prism-

box, and the large aperture D being brought into the centre of the substage. When it is desired to examine the spectrum of any portion of an object in the field of view, all that is necessary is to push the slit into adjustment with one hand, and the prisms with the other. The spectrum of any object which is superposed on the image of the slit is then seen.

The small square aperture at O (fig. 1) is for the examination of dichroic substances. When this is pushed into the field, by placing a double-image prism P between A B and E, two images of the aperture are seen in juxtaposition, oppositely polarized; and if a dichroic substance is on the stage, the differences of colour are easily seen.

When the spectrum of any substance is in the field and the double-image prism P is introduced, two spectra are seen, one above the other, oppositely polarized, and the variations in the absorption-lines, such as are shown by didymium, jargonium, &c., are at once seen.

A Nicol's prism, Q, as polarizer, is also arranged to slip into the same position as the double-image prism, and another, R, as analyzer, above the prism-box. The spectra of the brilliant colours exhibited by certain crystalline bodies, when seen by polarized light, can then be examined. Many curious effects are then produced, a description of which I propose to make the subject of another paper. Both the prisms P and Q are capable of rotation.

If the substance under examination is dark coloured, or the illumination is not brilliant, it is best not to divide the light by means of the Wenham prism at N, but to let the whole of it pass up the tube to one eye. If, however, the light is good, a very great advantage is gained by throwing the Wenham prism into adjustment and using both eyes. The appearance of the spectrum, and the power of grasping faint lines, are incomparably superior when both eyes are used; whilst the stereoscopic effect it confers on some absorption and interference spectra (especially those of opals) seems to throw entirely new light on the phenomena. No one who has worked with a stereoscopic spectrum-apparatus would willingly return to the old monocular spectroscope*.

If the illumination in this instrument is taken from a white cloud or the sky, Fraunhofer's lines are beautifully visible; and when using direct sunlight they are seen with a perfection which leaves little to be desired. The dispersion is sufficient to cause the spectrum to fill the whole field of the microscope, instead of, as in the ordinary instrument, forming a small portion of it, the dispersion being four or five times as great; whilst, owing to the very perfect achromatism of the optical part of the microscope, all the lines from B to G are practically in the same focus.

As the only portion of the object examined is that part on which the image of the slit falls, and as this is very minute (varying from

* It is not difficult to convert an ordinary spectroscope into a binocular instrument. The rays after leaving the object-glass of the telescope are divided into two separate bundles and received on two eyepieces properly mounted. As it is immaterial whether the spectrum be stereoscopic or pseudoscopic, a simpler form of prism than Mr. Wenham's arrangement can be used.

0·01 to 0·001 inch, according to the actual width of the slit), it is evident that the spectrum of the smallest objects can be examined. If some blood is in the field, it is easy to reduce the size of the image of the slit to dimensions covered by one blood-disk, and then, by pushing in the prisms, to obtain its spectrum.

If the object under examination will not transmit a fair image of the slit (if it be a rough crystal of jargoon for instance), it must be fixed in the universal holder beneath the slit and the light concentrated on it before it reaches the slit. If the spectra of opaque objects are required, they can also be obtained in the same way, the light being concentrated on them either by a parabolic reflector or by other appropriate means.

By replacing the illuminating lamp by a spirit-lamp burning with a soda-flame, and pushing in the spectrum-apparatus, the yellow sodium-line is seen beautifully sharp; and by narrowing the slit sufficiently it may even be doubled. Upon introducing lithium- or thallium-compounds into the flame, the characteristic crimson or green line is obtained; in fact so readily does this form of instrument adapt itself to the examination of flame-spectra, that for general work I have almost ceased to use a spectroscope of the ordinary form. The only disadvantage I find is an occasional deficiency of light; but by an improved arrangement of condensers I hope soon to overcome this difficulty.

"On some Optical Phenomena of Opals." By William Crookes, F.R.S. &c.

When a good fiery opal is examined in day-, sun-, or artificial light, it appears to emit vivid flashes of crimson, green, or blue light, according to the angle at which the incident light falls, and the relative position of the opal and the observer; for the direction of the path of the emitted beam bears no uniform proportion to the angle of the incident light. Examined more closely, the flashes of light are seen to proceed from planes or surfaces of irregular dimensions inside the stone, at different depths from the surface and at all angles to each other. Occasionally a plane emitting light of one colour overlaps a plane emitting light of another colour, the two colours becoming alternately visible upon slight variations of the angle of the stone; and sometimes a plane will be observed which emits crimson light at one end, changing to orange, yellow, green, &c., until the other end of the plane shines with a blue light, the whole forming a wonderfully beautiful solar spectrum in miniature. I need scarcely say that the colours are not due to the presence of any pigment, but are interference colours caused by minute striæ or fissures lying in different planes. By turning the opal round and observing it from different directions, it is generally possible to get a position in which it shows no colour whatever. Viewed by transmitted light, opals appear more or less deficient in transparency and have a slight greenish yellow or reddish tinge.

In order to better adapt them to the purposes of the jeweller, opals are almost always polished with rounded surfaces, back and front;

but the flashes of coloured light are better seen and examined when the top and bottom of the gems are ground and polished flat and parallel.

A good opal is not injured by moderate heating in water, soaking in turpentine, or heating strongly in Canada balsam and mounting as a microscopic slide.

By the kindness of Mr. W. Chapman, of Frith Street, Soho, and other friends, I have been enabled to submit some thousands of opals to optical examination ; and from these I have selected about a dozen which appeared worthy of further study.

If an opal which emits a fine broad crimson light is held in front of the slit of a spectroscope or spectrum-microscope, at the proper angle, the light is generally seen to be purely homogeneous, and all the spectrum that is visible is a brilliant luminous line or band, varying somewhat in width and more or less irregular in outline, but very sharp, and shining brightly on a perfectly black ground. If, now, the source of light is moved, so as to shine into the spectrum-apparatus *through* the opal, the above appearance is reversed, and we have a luminous spectrum with a jet-black band in the red, identical in position, form of outline, and sharpness with the luminous band previously observed. If instead of moving the first source of light (the one which gave the reflected luminous line in the red) another source of light be used for obtaining the spectrum, the two appearances, of a coloured line on a black ground, and a black line on a coloured ground, may be obtained simultaneously, and they will be seen to fit accurately.

Those parts of the opal which emit red light are therefore seen to be opaque to light of the same refrangibility as that which they emit; and upon examining in the same manner other opals which shine with green, yellow, or blue light, the same appearances are observed, showing that this rule holds good in these cases also. It is doubtless a general law, following of necessity the mode of production of the flashes of colour.

Having once satisfied myself that the above law held good in all the instances which came under my notice, I confined myself chiefly to the examination of the transmitted spectra, although the following descriptions will apply equally well, *mutatis mutandis*, to the reflected spectra. The examinations were made by means of the spectrum-microscope, which instrument is peculiarly adapted to examinations of this sort, both on account of the small size of the object which can be examined in it, and also as it permits the use of both eyes in viewing the spectrum.

The following is a brief description of some of the most curious transmission spectra shown by these opals. The accompanying figures, drawn with the camera lucida, convey as good an idea as possible of the different appearances. The exact description will of course only hold good for one portion of the opal; but the general character of each individual stone is well marked.

No. 1 shows a single black band in the red. When properly in focus this has a spiral structure. Examined with both eyes it appears

in decided relief, and the arrangement of light and shade is such as to produce a striking resemblance to a twisted column.

No 2. gives an irregular line in the orange. Viewed binocularly, this exhibits the spiral structure in a marked manner, the different depths and distances standing well out; upon turning the milled head of the stage-adjustment, so as to carry the opal slowly from left to right, the spiral line is seen to revolve and roll over, altering its shape and position in the spectrum. It is not easy to retain the conviction that one is looking merely at a band of deficient light in the spectrum, and not at a solid body, possessing dimensions and in actual motion.

No. 3 has a line between the yellow and green, vanishing to a point at the top, and near the bottom having a loop, in the centre of which the green appears. Higher up, in the green, is a broad green band, indistinct on one side and branching out in different parts.

No. 4 has a broad, indistinct, and sloping band in the blue, and another, still more indistinct, in the violet.

No. 5 has a band in the yellow, not very sharp on one side, and somewhat sloping. Upon moving the opal sideways, it moves about from one part of the yellow field to another. In one position it covers the line D, and is opaque to the sodium-flame of a spirit-lamp.

No. 6 shows a curiously shaped band in the red, very sharp and black, and terminating in one part at the line D. In the yellow there is a black dot. The spectrum of this opal showed by reflected light intensely bright red bands, of the shape of the transmission bands. On examining this opal with a power of 1 inch, in the ordinary manner, the portion giving this spectrum appeared to glow with intense red light, and was bounded with a tolerably definite outline. Without altering any other part of the microscope, the prisms were then pushed in so as to look at the whole surface of the opal through the prisms, but without the slit. The shape and appearance of the red patch were almost unaltered; and here and there over other parts of the opal were seen little patches of homogeneous light, which, not having been fanned out by the prisms, retained their original shape and appearance.

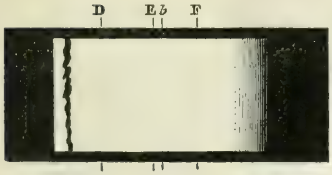
No. 7 shows a black patch in the red, only extending a little distance, and a line in the yellow. On moving the opal the line in the red vanishes, and the other line changes its position and form.

No. 8 shows the most striking example of a spiral rotating line which I have yet met with. On moving the opal sideways the line is seen to start from the red and roll over, like an irregularly shaped and somewhat hazy corkscrew, into the middle of the yellow. The drawing shows the appearance of this band in two positions.

No. 9 is one of the most curious. A broad black and sharp band stretches diagonally across the green, touching the blue at the top and the yellow at the bottom.

No. 10 gives a diagonal band, wide, but straight, and tolerably sharp across the green. By rotating these opals, 9 and 10, in azimuth, whilst in the field of the instrument, the lines can be made to

No. 1.



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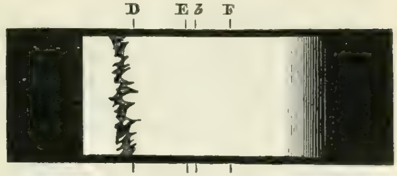

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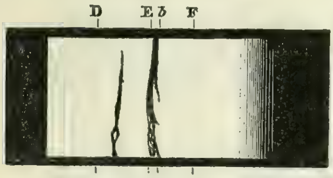

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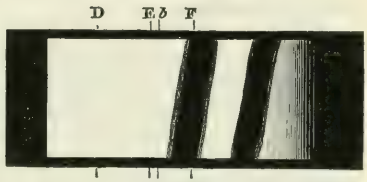

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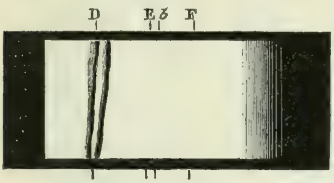

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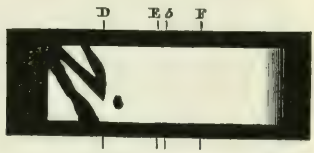

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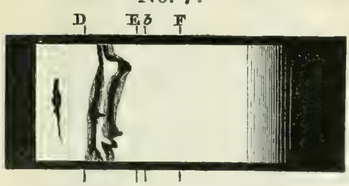

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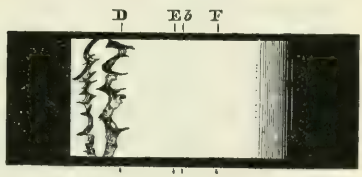
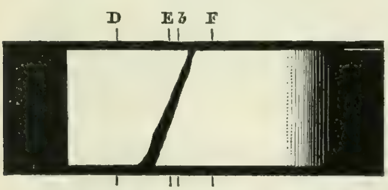
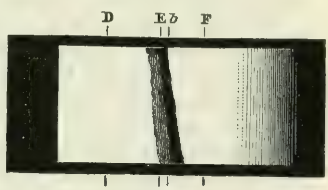


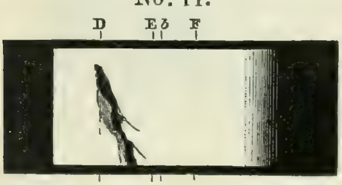
No. 9.



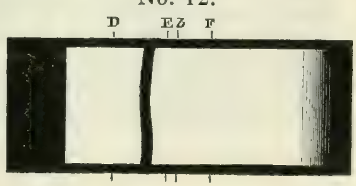
No. 10.



No. 11.



No. 12.



alter in inclination until they are seen to slope in the opposite direction.

No. 11 gives another illustration of a diagonal line, across the yellow and green, not extending quite to the top.

No. 12 is one of the best examples I have met with of a narrow, straight, and sharply cut line. It is in the green, and might easily be mistaken for an absorption-band caused by an unknown chemical element.

Other opals are exhibited, which show a dark band travelling along the spectrum, almost from one end to the other, as the opal is moved sideways.

It is scarcely necessary to say that the colour of the moving luminous line varies with the part of the spectrum to which it belongs. The appearance of a luminous line, slowly moving across the black field of the instrument, and assuming in turn all the colours of the spectrum, is very beautiful.

All these black bands can be reversed, and changed into luminous bands, by illuminating the opal with reflected light. They are, however, more difficult to see; for the coloured light is only emitted at a particular angle, whilst the special opacity to the ray of the same refrangibility as the emitted ray holds good for all angles.

The explanation of the phenomena is probably as follows:—In the case of the moving line, the light-emitting plane in the opal is somewhat broad, and has the property of giving out at one end, along its whole height and for a width equal to the breadth of the band, say, red light; this merges gradually into a space emitting orange, and so on throughout the entire length of the spectrum, or through that portion of it which is traversed by the moving line in the instrument, the successive pencils (or rather ribbons) of emitted light passing through all degrees of refrangibility. It is evident that if this opal is slowly passed across the slit of the spectrum-microscope, the slit will be successively illuminated with light of gradually increasing refrangibility, and the appearance of a moving luminous line will be produced; and if transmitted light is used for illumination, the reversal of the phenomena will cause the production of a black line moving along a coloured field. A diagonal line will be produced if an opal of this character is examined in a sloping position.

The phenomenon of a spiral line in relief, rolling along as the opal is moved, is doubtless caused by modifying planes at different depths and connected by cross planes; I can form a mental picture of a structure which would produce this effect, but not clear enough to enable me to describe it in words.

It is probable that similar phenomena may be seen in many, if not all, bodies which reflect coloured light after the manner of opals. A magnificent specimen of Lumacelli, or Fiery Limestone, from Italy, kindly presented to me by my friend David Forbes, shows two sharp narrow and parallel bands in the red. I have also observed similar appearances in mother-of-pearl. The effects can be imitated to a certain extent by examining "Newton's rings," formed between two plates of glass, in the spectrum-instrument.

June 10.—Lieut.-General Sabine, President, in the Chair.

The following communications were read:—

"On a new Astronomical Clock, and a Pendulum-governor for Uniform Motion." By Sir William Thomson, LL.D., F.R.S.

It seems strange that the dead-beat escapement should still hold its place in the astronomical clock, when its geometrical transformation, the cylinder escapement of the same inventor, Graham, only survives in Geneva watches of the cheaper class. For better portable time-keepers, it has been altered (through the rack-and-pinion movement) into the detached lever, which has proved much more accurate. If it is possible to make astronomical clocks go better than at present by merely giving them a better escapement, it is quite certain that one on the same principle as the detached lever, or as the ship-chronometer escapement, would improve their time-keeping.

But the inaccuracies hitherto tolerated in astronomical clocks may be due more to the faultiness of the mercury compensation pendulum, and of the mode in which it is hung, and of the instability of the supporting clock-case or framework, than to imperfection of the escapement and the greatness of the arc of vibration which it requires; therefore it would be wrong to expect confidently much improvement in the time-keeping merely from improvement of the escapement. I have therefore endeavoured to improve both the compensation for change of temperature in the pendulum, and the mode of its support, in a clock which I have recently made with an escapement on a new principle, in which the simplicity of the dead-beat escapement of Graham is retained, while its great defect, the stopping of the whole train of wheels by pressure of a tooth upon a surface moving with the pendulum, is remedied.

Imagine the escapement-wheel of a common dead-beat clock to be mounted on a collar fitting easily upon a shaft, instead of being rigidly attached to it. Let friction be properly applied between the shaft and the collar, so that the wheel shall be carried round by the shaft unless resisted by a force exceeding some small definite amount, and let a governor giving uniform motion be applied to the train of wheel-work connected with this shaft, and so adjusted that, when the escapement-wheel is unresisted, it will move faster by a small percentage than it ought to move when the clock is keeping time properly. Now let the escapement-wheel, thus mounted and carried round, act upon the escapement, just as it does in the ordinary clock. It will keep the pendulum vibrating, and will, just as in the ordinary clock, be held back every time it touches the escapement during the interval required to set it right again from having gone too fast during the preceding interval of motion. But in the ordinary clock the interval of rest is considerable, generally greater than the interval of motion. In the new clock it is equal to a small fraction of the interval of motion: $\frac{1}{300}$ in the clock as now working, but to be reduced probably to something much smaller yet. The simplest appliance to count the turns of this escapement-wheel (a worm, for instance, working upon a wheel with thirty teeth, carrying a hand round, which will

correspond to the seconds' hand of the clock) completes the instrument; for minute- and hour-hands are a superfluity in an astronomical clock.

In various trials which I have made since the year 1865, when this plan of escapement first occurred to me, I have used several different forms, all answering to the preceding description, although differing widely in their geometrical and mechanical characters. In all of them the escapement-wheel is reduced to a single tooth or arm, to diminish as much as possible the moment of inertia of the mass stopped by the pendulum. This arm revolves in the period of the pendulum (two seconds for one second's pendulum), or some multiple of it. Thus the pendulum may execute one or more complete periods of vibration without being touched by the escapement.

I look forward to carrying the principle of the governed motion for the escapement-shaft much further than hitherto, and adjusting it to gain only $\frac{1}{100}$ per cent. on the pendulum; and then I shall probably arrange that each pallet of the escapement be touched only once a minute (and the counter may be dispensed with). The only other point of detail which I need mention at present is that the pallets have been, in all my trials, attached to the bottom of the pendulum, projecting below it, in order that satisfactory action with a very small arc of vibration (not more on each side than $\frac{1}{100}$ of the radius, or 1 centimetre for the second's pendulum) may be secured.

My trials were rendered practically abortive from 1865 until a few months ago by the difficulty of obtaining a satisfactory governor for the uniform motion of the escapement-shaft; this difficulty is quite overcome in the pendulum-governor, which I now proceed to describe.

Imagine a pendulum with single-tooth escapement mounted on a collar loose on the escapement-shaft just as described above—the shaft, however, being vertical in this case. A square-threaded screw is cut on the upper quarter of the length of the shaft, this being the part of it on which the collar works, and a pin fixed to the collar projects inwards to the furrow of the screw, so that, if the collar is turned relatively to the shaft, it will be carried along, as the nut of a screw, but with less friction than an ordinary nut. The main escapement-shaft just described is mounted vertically. The lower screw and long nut collar, three-quarters of the length of the escapement-shaft, are surrounded by a tube which, by wheelwork, is carried round about five per cent. faster than the central shaft. This outer shaft, by means of friction produced by the pressure of proper springs, carries the nut collar round along with it, except when the escapement-tooth is stopped by either of the pallets attached to the pendulum. A stiff cross piece (like the head of a T), projecting each way from the top of the tubular shaft, carries, hanging down from it, the governing masses of a centrifugal friction governor. These masses are drawn towards the axis by springs, the inner ends of which are acted on by the nut collar, so that the higher or the lower the latter is in its range, the springs pull the masses inwards with less or more force. A fixed metal ring coaxial with the main shaft

holds the governing masses in when their centrifugal forces exceed the forces of the springs, and resists the motion by forces of friction increasing approximately in simple proportion to the excess of the speed above that which just balances the forces of the springs. As long as the escapement-tooth is unresisted, the nut collar is carried round with the quicker motion of the outer tubular shaft, and so it *screws upwards*, diminishing the force of the springs. Once every semiperiod of the pendulum it is held back by either pallet, and the nut collar screws *down* as much as it rose during the preceding interval of freedom when the action is regular; and the central or main escapement-shaft turns in the same period as the tooth, being the period of the pendulum. If through increase or diminution of the driving-power, or diminution or increase of the coefficient of friction between the governing masses and the ring on which they press, the shaft tends to turn faster or slower, the nut collar works its way down or up the screw, until the governor is again regulated, and gives the same speed in the altered circumstances. It is easy to arrange that a large amount of regulating power shall be implied in a single turn of the nut collar relatively to the central shaft, and yet that the periodic application and removal of about $\frac{1}{50}$ of this amount in the half period of the pendulum shall cause but a *very small* periodic variation in the speed. The latter important condition is secured by the great moment of inertia of the governing masses themselves round the main shaft. I hope, after a few months' trial, to be able to present a satisfactory report of the performance of the clock now completed according to the principles explained above. As many of the details of execution may become modified after practical trial, it is unnecessasy that I should describe them minutely at present. Its general appearance, and the arrangement of its characteristic parts, may be understood from the photograph now laid before the Society.

June 17.—Lieut.-General Sabine, President, in the Chair.

The following communication was read:—

"Note upon a Self-registering Thermometer adapted to Deep-sea Soundings." By W. A. Miller, M.D., Treas. and V.P.R.S.

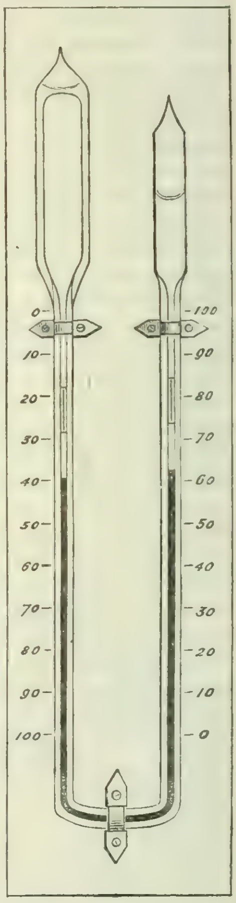
The Fellows of the Royal Society are already aware that the Admiralty, at the request of the Council of the Society, have placed a surveying-vessel at the disposal of Dr. Carpenter and his coadjutors for some weeks during the present summer, to enable them to institute certain scientific inquiries in the North Sea. Among the objects which the expedition has in view is the determination of deep-sea temperatures.

Now it is well known that self-registering thermometers of the ordinary construction are liable to error when sunk to considerable depths in water, in consequence of the diminution produced for the time in the capacity of the bulb under the increased pressure to which it is subjected. The index, from this cause, is carried forward beyond

the point due to the effect of mere temperature, and the records furnished by the instrument rise too high*.

A simple expedient occurred to me as being likely to remove the difficulty; and as upon trial it was found to be perfectly successful, I have thought that a notice of the plan pursued might not be unacceptable to future observers.

The form of self-registering thermometer which it was decided to employ is one constructed upon Six's plan. Much care is requisite in adjusting the strength of index-spring, and the size of the pin, so as to allow it to move with sufficient freedom when pressed by the mercury, without running any risk of displacement in the ordinary use of the instrument while raising or lowering it into the water. Several of these thermometers have been prepared for the purpose with unusual care by Mr. Casella, who has determined the conditions of strength in the spring and diameter of tube most favourable to accuracy. He has also himself had an hydraulic press constructed expressly with the view of testing these instruments. By means of this press the experiments hereafter to be described were made.



The expedient adopted for protecting the thermometers from the effects of pressure consisted simply in enclosing the bulb of such a Six's thermometer in a second or outer glass tube, which was fused upon the stem of the instrument in the manner shown in the accompanying figure. This outer tube was nearly filled with alcohol, leaving a little space to allow of variation in bulk due to expansion. The spirit was heated to displace part of the air by means of its vapour, and the outer tube and its contents were sealed hermetically.

In this way, variations in external pressure are prevented from affecting the bulb of the thermometer within, whilst changes of temperature in the surrounding medium are speedily transmitted through the thin stratum of interposed alcohol. The thermometer is protected from external injury by enclosing it in a suitably constructed copper case, open at top and bottom, for the free passage of the water.

In order to test the efficacy of this plan, the instruments to be tried were enclosed

* In sea-water of sp. gr. 1·027, the pressure in descending increases at the rate of 280 lbs. upon the square inch for every 100 fathoms, or exactly one ton for every 800 fathoms.

in a strong wrought-iron cylinder filled with water, and submitted to hydraulic pressure, which could be raised gradually till it reached three tons upon the square inch; and the amount of pressure could be read as the experiment proceeded, upon a gauge attached to the apparatus.

Some preliminary trials made upon the 5th of May showed that the press would work satisfactorily, and that the form of thermometer proposed would answer the purpose.

These preliminary trials showed that, even in the thermometers with protected bulbs, a forward movement of the index of from 0°·5 to 1° F. occurred during each experiment. This, however, I believed was caused, not by any compression of the bulb, but by a real rise of temperature, due to the heat developed by the compression of the water in the cavity of the press.

This surmise was shown to be correct by some additional experiments made last week to determine the point. On this occasion the following thermometers were employed:—

No. 9645. A mercurial maximum thermometer, on Prof. Phillips's plan, enclosed in a strong outer tube containing a little spirit of wine, and hermetically sealed.

No. 2. A Six's thermometer, with the bulb *protected*, as proposed by myself, with an outer tube.

No. 5. A Six's thermometer, with a long recurved cylindrical bulb, also *protected* in a similar manner.

No. 1. Six's thermometer, with cylindrical bulb of extra thickness, *not protected*.

No. 3. Six's thermometer, with spherical bulb, extra thick glass, *not protected*.

No. 6. Admiralty instrument, Six's thermometer, ebonite scale, bulb *not protected*.

No. 9651. An ordinary Phillips's maximum mercurial thermometer, spherical bulb, *not protected*.

The hydraulic press was exposed in an open yard, and had been filled with water several hours before. A maximum thermometer, introduced into a wrought-iron tube filled with water, open at one end to the outer air, closed at the other, where it passed into the water contained in the press, registered 46°·7 at the commencement, and 47° at the end of the experiment. Temperature of the external air 49° F.

In commencing the experiment, the seven thermometers under trial were introduced into the water in the cavity of the press, and after a lapse of ten minutes the indices of each were set, carefully read, and each instrument was immediately replaced in the press, which was then closed, and by working the pump the pressure was gradually raised to $2\frac{1}{2}$ tons upon the inch. It was maintained at this point for forty minutes, in order to allow time for the slight elevation of temperature caused by the compression of the water to equalize itself with that of the body of the apparatus. At the end of the forty minutes the pressure was rapidly relaxed. A corresponding depression of temperature was thus occasioned, the press was opened im-

mediately, and the position of the indices of each thermometer was again read carefully; and the water was found to be at a temperature sensibly lower than before the experiment began, by about 0°·6 F. By this means it was proved that the forward movement of the index in the protected thermometers, amounting to 0°·9, was really due to temperature, and not to any temporary change in the capacity of the bulb produced by pressure.

This will be rendered evident by an examination of the subjoined Table of observed temperatures:—

First Series: Pressure $2\frac{1}{2}$ tons per square inch.

Number of Thermometer.	Minimum index.		Maximum index.		Maximum mercury. After.
	Before.	After.	Before.	After.	
Protected ... 9645	......	......	47·0	47·7	
,, ... 2	47·0	46·5	46·7	47·6	46·5
,, ... 5	47·0	46·3	46·5	47·6	46·0
Mean......	......	......	......	47·6	
Unprotected. 1	46·7	46·4	46·5	54·0	46
,, 3	47·0	46·5	46·5	56·5	46
,, 56	47·0	46·0	47·0	55·5	46
,, 9651	......	......	46·7	118·5	
Mean......	46·9	46·3	46·7	......	46·1
Temperature of external air......			49	49	
Temperature of thermometer in press			46·7	47	

In the Phillips's maximum thermometer, with unprotected spherical bulb, No. 9651, the bulb had experienced so great a degree of compression as to drive the index almost to the top of the tube. In all the other unprotected instruments, which had been made with bulbs of unusual thickness, the index had been driven beyond its proper position from 6°·4 to 8°·9 F.; and it is obvious that the amount of this error must vary in each instrument with the varying thickness of the bulb and its power of resisting compression.

Notwithstanding the great pressure to which these instruments had been subjected, all of them, without exception, recovered their original scale-readings as soon as the pressure was removed.

It will be seen that the mean rise of temperature indicated by the three protected instruments was 0°·9 F., whilst the mean depression registered on removing the pressure amounted upon all the instruments which admitted of its measurement to 0°·6, an agreement as close as was to be expected from the conditions of the experiment.

A second set of experiments was made upon the same set of instruments, with the exception of 9651; but the pressure was now raised to 3 tons upon the inch; this was maintained for ten minutes. When

it had risen to $2\frac{3}{4}$ tons a slight report was heard in the press, indicating the fracture of one of the thermometers. On examining the contents of the press afterwards it was found that No. 2 was broken; the others were uninjured. The broken thermometer was the earliest constructed upon the plan now proposed, and it was consequently not quite so well finished as subsequent practice has secured for those of later construction. The results of the trial under the higher pressures showed an increase in the amount of compression experienced by the unprotected instruments rising in one instance to as much as 11°·5 F. With the protected instruments the rise did not exceed 1°·5, due, as before, to the heat evolved from the water by its compression.

A pressure of 3 tons, it may be observed, would be equal to that of 448 atmospheres of 15 lb. upon the square inch; and if it be assumed that the diminution in bulk of water under compression continues uniformly at the rate of 47 millionths of its bulk for each additional atmosphere, the reduction in bulk of water under a pressure of 3 tons upon the square inch will amount to about $\frac{1}{47}$ of its original volume. This probably is too high an estimate, as the rate of diminution would most likely decrease as the pressure increases.

GEOLOGICAL SOCIETY.

[Continued from p. 322.]

February 24th, 1869,—Prof. T. H. Huxley, LL.D,, F.R.S., President, in the Chair.

The following communication was read:—

"On the British Postglacial Mammalia." By W. Boyd Dawkins, Esq., M.A., F.R.S., F.G.S.

The author stated that the Postglacial or Quaternary Mammalia of England and Wales amounted to 47. Of these only 15 are found in Caves and not in River deposits, whilst out of 31 found in the latter, only 1 does not occur in caves; hence the author inferred that the Cave and River deposits are palæontologically synchronous. In Scotland, remains of Mammalia have occurred only in five places, and in Ireland only in two places, in beds of Postglacial age. The author ascribed this unequal distribution to the long continuance of subaërial glaciation in Ireland, Scotland, and North Wales.

The author then compared the Postglacial with the Preglacial Mammalia. The British species of the latter are:—

Ursus arvernensis.	*Bos primigenius.*
—— *spelæus*?.	*Hippopotamus major.*
Sorex.	*Equus fossilis.*
Mygale moschata.	*Rhinoceros megarhinus.*
Talpa europæa.	—— *Etruscus.*
Cervus megaceros!	*Elephas antiquus.*
—— *capreolus.*	—— *meridionalis.*
—— *elaphus.*	*Arvicola amphibia.*
—— Sedgwickii.	*Castor fiber.*
—— Ardeus.	Trogontherium Cuvieri.

Of these 19 species inhabiting Britain before the deposition of the Boulder-clay, 13 survived into Postglacial times*.

Passing from Postglacial to Prehistoric time, the Sheep, Goat, *Bos longifrons*, and Dog make their appearance, while the great Pachydermata, the Cave Mammals, and nearly all the northern forms disappear. The characteristic postglacial mommals were defined by the author to be

Palæolithic man.	*Ovibos moschata.*
Gulo luscus.	*Rhinoceros tichorhinus.*
Ursus spelæus?	*Elephas primigenius.*
—— *ferox.*	*Lemmus.*
Felis leo.	*Spermophilus citillus.*
—— *pardus.*	—— *erythrogenoides.*
Hyæna spelæa.	

The author finally discussed the question of the age of the Lower Brick-earths of the Thames valley and Clacton, and indicated the difficulty of proving, from Palæontological evidence, whether they are pre- or postglacial. He supposed that durihg the glacial submergence, the valley of the Lower Thames roughly marked the coast-line of the icy sea, with a climate too cold to allow the continued residence of the Preglacial mammals, but which might still occasionally be visited by their surviving descendants, the remains of which would thus be mingled with those of Arctic immigrants.

March 10th, 1869.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Origin of the Northampton Sand." By John W. Judd, Esq., F.G.S., of the Geological Survey of England.

This paper was an attempt to base on the study of a rock, both in the field and the laboratory, a complete and consistent theory of the conditions of its original deposition, and of the sequence and causes of its various metamorphoses.

The Northampton Sand was described as consisting of various strata, usually of an arenaceous character, which frequently pass, both vertically and horizontally, into a ferruginous rock, the well-known Northamptonshire ore.

The different features presented by the formation in various localities were then indicated; and the lithological, microscopical, and chemical characters of its constituent rocks described at length.

These characters were shown to point to the conclusion that the beds were accumulated in a delta of one or more great rivers.

Arguments were then adduced in opposition to the theory of the formation of ironstones by direct deposition, and in favour of the hypothesis that the Northamptonshire ore consisted of beds of sand altered by the percolation through them of water containing carbonate of iron.

The cause of the redistribution of the iron in the rock was then discussed; and, in opposition to the views of Mr. Maw, who has

* The names of these are printed in italic.

referred the phenomena in question to "*segregation*," they were all shown to be easily capable of explanation on well-known chemical principles, and to be due to the action of atmospheric water finding access to the rock by its joints and fissures.

The paper concluded with a sketch of what was inferred to be the history of the rock from its accumulation to the present time, and some remarks on the varied and important effects of water when acting under different conditions on rocks.

2. "On the Occurrence of Remains of *Pterygotus* and *Eurypterus* in the Upper Silurian Rocks in Herefordshire." By the Rev. P. B. Brodie, M.A., F.G.S.

In this paper the author described the occurrence of numerous specimens of Crustacea, chiefly belonging to the genera *Eurypterus* and *Pterygotus*, in beds of Upper Silurian age, probably the "passage beds," in the Woolhope district and near Ludlow.

March 24th, 1869.—Sir Philip de M. Grey Egerton, Bart., M.P., F.R.S., in the Chair.

The following communications were read:—

1. "On the Cretaceous Strata of England and the North of France, compared with those of the West, South-west, and South of France, and the North of Africa." By Professor Henri Coquand, of Marseilles.

In this paper the author indicated that the agreement between the Cretaceous strata of England and the North of France, as far as the Basin of Paris, is such that the same classification may be applied to the whole, but that in advancing to the west and south new beds make their appearance. This is also the case in Algeria, the palæontological differences between the Cretaceous rocks of that country and those of the Anglo-Parisian basin being so great as to lead at first sight to the impression that they belong to two different formations. The author arrived at the following classification and nomenclature of the divisions of the Cretaceous rocks, the palæontological characters and geographical range of which were described in the paper:—

I. Upper Cretaceous.

A. Red Lancustrine Sandstone of Vitrolles (=Garumnien of Leymerie).
B. Dordonien.
C. Campanien (=Upper Chalk).
D. Santonien (=Superior Lower Chalk).
E. Coniacien (Sandstone).

II. Middle Cretaceous.

F. Provencien.
G. Mornasien.
H. Angoumien.
I. Ligérian (=Inferior Lower Chalk).
J. Carentonien.
K. Gardonien.
L. Rothomagien (=Upper Greensand and Chalk-marl).
M. Gault.

III. Lower Cretaceous.

N. Aptien.
1. Upper.
2. Middle } = Lower Greensand.
3. Lower }
O. Neocomien.
P. Valengien.

2. "On the Structure and Affinities of *Sigillaria* and allied genera." By W. Carruthers, Esq., F.L.S., F.G.S.

The author indicated the characters of the medullary rays of dicotyledonous stems, and stated that these stems have a vascular horizontal system connected with the axial organs, in which respect the dicotyledonous and acrogenous stems agree. The woody columns of *Stigmaria* and *Sigillaria* are destitute of medullary rays, the structures previously described as such being the vascular bundles running to the rootlets and leaves. Hence the author concluded that *Sigillaria* is a true cryptogam—a position supported by the characters of the organs of reproduction as described by Goldenberg. The paper concluded with an enumeration of the forms of fruits belonging to *Sigillaria* and its allied genera, with indications of the existing forms to which they most nearly approach.

3. "On the British Species of the Genera *Climacograpsus*, *Diplograpsus*, *Dicranograpsus*, and *Didymograpsus*." By H. Alleyne Nicholson, D.Sc., M.B., F.G.S.

The author stated that all the genera referred to in this paper appear to be exclusively of Lower Silurian age,—*Climacograpsus* and *Diplograpsus* occurring almost throughout the Lower Silurian series, whilst the other two genera belong chiefly to the Llandeilo series of rocks, or to strata of corresponding position out of Britain.

The British species of the above genera admitted by the author are:—

Climacograpsus teretiusculus (*His.*).
—— bicornis (*Hall*).
—— tuberculatus, *Nich.*, sp. n.
Diplograpsus pristis (*His.*).
—— mucronatus (*Hall*).
—— Whitfieldii (*Hall*).
—— Harknessii, *Nich.*
—— confertus, *Nich.*
—— cometa, *Gein.*
—— palmeus, *Barr.*
—— acuminatus, *Nich.*
—— vesiculosus, *Nich.*
—— pristiniformis (*Hall*).
Diplograpsus tamariscus, *Nich.*
—— putillus (*Hall*).
—— nodosus, *Harkn.*
—— pinnatus, *Harkn.*
——, sp.
Dicranograpsus ramosus (*Hall*).
Didymograpsus Murchisoni (*Beck*).
—— affinis, *Nich.*, sp. n.
—— divaricatus (*Hall*).
—— anceps, *Nich.*
—— flaccidus (*Hall*).
—— sextans (*Hall*).

The paper included descriptions of the supposed embryonic states of several of the species.

April 14th, 1869.—Prof. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Coal-mines at Kaianoma, in the Island of Yezo." By F. O. Adams, Esq., Hon. Secretary of Legation in Japan.

The writer states that the works at Kaianoma have made con-

siderable progress since they were reported upon by Mr. Mitford last year *. There are four seams of coal, each about 7 feet thick, from 50 to 100 feet apart. A tunnel has been driven through one of the seams for a distance of between 150 and 250 feet, and at an elevation of 430 feet above the sea. From this the coal obtained is carried down to the shore on the backs of men, mules, and ponies. The writer adds that there is abundance of coal "of the cannel description."

2. "On a peculiarity of the Brendon-Hills Spathose Ore-veins." By M. Morgans, Esq.

The author described the Brendon Hills as consisting of a Devonian slate dipping S. by E. and N. by W. on the two sides of the axis of elevation. The cleavage-laminæ dip S. by W. at an angle of 80°; and the cleavage-strike forms only a slight angle with that of the beds, which, however, is sometimes irregular. Veins of spathose iron-ore, very rich in manganese, occur in the slate; and the general dip of these appears to coincide with that of the cleavage-planes. The veins consist of thin "tracks" of softened clay-slate and quartz, with larger or smaller pockets of productive ore. These metalliferous portions do not descend parallel to the line of their dip, but slope more or less, usually to the west. The author stated that the veins have been segregated from the adjoining clay-slate, the unproductive portions of them occurring where the conterminous strata were not impregnated with sufficient ferruginous matter to produce a lode of iron-ore; the slope of each productive part, called "end-slant" by the author, is determined by the line of intersection of the plane of the vein with the boundaries of the ferruginous portions of the beds.

XLVI. *Intelligence and Miscellaneous Articles.*

ON THE EMISSION AND ABSORPTION OF HEAT RADIATED AT LOW TEMPERATURES. BY G. MAGNUS.

1. DIFFERENT substances, when heated to 150° C., emit different kinds of heat.

2. There are bodies which emit only one kind of heat, and others which emit several.

3. To the first class belongs rock-salt when it is quite pure. Just as the ignited vapour of this substance, or of one of its constituents (sodium), only emits *one* colour, so, too, it only radiates one kind of heat. It is monothermal, as its vapour is monochromatic.

4. Rock-salt absorbs the heat radiated by rock-salt in larger quantity, and more energetically, than that of sylvine (chloride of potassium) and other kinds of heat. Hence, contrary to what Melloni

* See Quart. Journ. Geol. Soc. vol. xxiv. p. 511.

and Knoblauch allege, it does not transmit all kinds of heat equally well.

5. Absorption by rock-salt increases with the thickness of the absorbing plate.

6. The great diathermancy of rock-salt does not depend upon a small absorbing-power for different kinds of heat, but upon the circumstance that it only emits one kind of heat and only absorbs this one, and that almost all other bodies at a temperature of 150° C. emit heat which only contains a small portion, or none at all, of the rays which rock-salt emits.

7. Sylvine behaves like rock-salt, but is not monothermal to the same extent. In this case also we have an analogy with its ignited vapours or those of potassium, which is known to give an almost continuous spectrum.

8. Fluor-spar absorbs the pure heat from rock-salt almost completely. It would thence be expected that the heat which it emits is also strongly absorbed by rock-salt; yet 70 per cent. passes through a rock-salt plate 20 millims. thick. Taking into consideration the sum of the heat which fluor-spar emits, which is more than thrice as much as that of rock-salt, this phenomenon might be explained; but it needs further investigation.

9. If it were possible to construct a spectrum of the heat radiated at 150° C., and if rock-salt were the substance, the spectrum would contain only *one* band. If sylvine were used for radiation the spectrum would be more extended, but would only occupy a small portion of that which would result from the heat radiated by lampblack. —*Berliner Monatsbericht*, June 1869.

ON THE LIMITS OF THE MAGNETIZATION OF IRON AND STEEL. BY PROF. A. WALTENHOFEN.

The author has subjected to exhaustive calculations the whole of the present materials of observation on the connexion between electromagnetism and current-intensity, and has thus arrived at the following result.

The limiting value of the magnetic momentum of the unit of weight corresponding to the condition of magnetic saturation of iron is an absolute constant (that is, independent of the shape and magnitude of the electromagnet) whose numerical value amounts to very nearly 2100 absolute units per milligramme.

It follows from this that the theoretically possible temporary magnetization of iron is more than five times as much as the permanent which has been attained by the best steel magnets, if, with M. Weber, we take the latter as 400 absolute units per milligramme.

The author considers it remarkable that just this degree of saturation is also that required by the law which he discovered in 1863, in reference to the temporary magnetization of steel bars by means of the electrical current; while, in the case of iron, Lenz and Jacobi's law of proportionality, as the author shows, only holds up to a degree of saturation of (on the average) 800 absolute units per milligramm

The author regards the absolute limiting value of the magnetic momentum of the unit of length as a physical constant characteristic of iron, and comparable with the constants of elasticity, solidity, &c.; and he holds that its existence is quite in accord with the theory of rotatory molecular magnets, of the probability of which he thinks a striking proof has been afforded by his discovery of abnormal magnetization and the phenomena connected therewith.

The author finally points out that the result of his calculation, contained in the above law, also justifies the conclusion that the proportionality indicated by Müller between the coefficient B of his formula and the length of the bar, but considered inaccurate and imperfectly established, must have general validity. At the same time the circumstances are mentioned to which it must be ascribed that both Müller and the author were led to doubt, from existing data, the applicability and universality of this formula.

The author refers to a research by Oberbeck which has recently appeared, of which he only heard after his investigation was finished: in it the question of the existence of an independent limiting value of the magnetic momentum of the unit of volume is discussed. But the author remarks that this research involves no change or completion of the results above adduced; for the *amount* of the limiting value is neither ascertained nor adduced, and the results of the experiments show too irregular a course to permit a *numerical* deduction of such a limiting value, although the existence of such a one seems to follow from two of the series of them.—*Sitzungsberichte der Kaiserlichen Akademie in Wien,* 1869, No .12.

ON THE REFLECTION OF HEAT FROM THE SURFACE OF FLUOR-SPAR AND OTHER BODIES. BY G. MAGNUS.

After succeeding in freeing the heat from various substances raised to 150° C. from the rays of the heating-flame and of other heating-bodies, it was possible to show, in the research laid before the Academy on June 9, that there are some bodies which only radiate one or at most a few wave-lengths, others which emit a greater number. Hence it seemed interesting to answer the question, what is the reflecting-power of these bodies? whether the same differences which are observed in reference to the absorption and transmission of heat by bodies that are identical as regards the action of light also occur in the reflection of heat.

Differences in reflecting-power can only definitely occur when rays are reflected which only contain one or a few wave-lengths. Such rays have been already obtained by using individual parts of a spectrum produced by a rock-salt prism, or by allowing the rays of a source of heat which radiates many wave-lengths (those of a lamp for instance) to pass through substances which only absorb a certain number. But there are very few substances which transmit rays of only one or of a few wave-lengths; and these are, moreover, of small intensity.

In spite of this difficulty, MM. La Provostaye and Desains showed in 1849* that, according as heat from a Locatelli's lamp has passed through glass or through rock-salt, various quantities are reflected by speculum-metal, silver, and platinum; and in the case of all reflecting surfaces, less was reflected of that which had passed through glass than of that through rock-salt.

The same inquirers have subsequently published a comprehensive series of experiments made with the heat of a lamp decomposed by means of a glass prism, in which it was shown that heat from the different parts of the spectrum is variously reflected. But they restricted their experiments to reflection from metallic surfaces, doubtless on account of the feeble intensity of the incident heat. Now that we possess in rock-salt a substance which only emits one or a few wave-lengths, and we also know other bodies which at the temperature of 150° C. radiate a limited number of wave-lengths, it is possible to make experiments on the reflection of non-metallic surfaces. It has thus been found that from these the different kinds of heat or wave-lengths are reflected in very different quantity. Only one of the most surprising examples shall be here mentioned. It refers to the reflecting-power of fluor-spar.

Of heat which very different substances radiate, there are reflected at an angle of 45° quantities which are indeed not equal, but which do not differ much from each other.

Silver, between	....	83	and	90	per cent.
Glass	,,	6	,,	14	,,
Rock-salt	,,	5	,,	12	,,
Fluor-spar	,,	6	,,	10	,,

Of the heat from rock-salt, fluor-spar reflects 28 to 30 per cent., while silver, glass, and rock-salt do not reflect larger proportions of this than of the other kinds of heat.

Here, as in the experiments on the transmission of heat, it has been confirmed that sylvine emits a large quantity of rock-salt heat, but at the same time emits other kinds of heat. And fluor-spar reflects 15–17 per cent. of sylvine-heat, consequently less than it reflects of rock-salt heat, and more than it does of that from the other radiating bodies.

If our eyes had the power of distinguishing the various wave-lengths of heat as well as the colours of light, fluor-spar would appear brighter than all other substances when the rays of rock-salt fell upon them. If the rays came from sylvine, fluor-spar would also appear brighter than all other bodies, but not so bright as with the radiation from rock-salt.

Melloni has taught us that various substances transmit very different quantities of heat, and that the source from which it originates has great influence on its transmission. But the sources of heat were only distinguished as to their degree of heat, and we knew that with increasing temperature the diversity of the radiation increased.

* *Comptes Rendus*, vol. xxviii. p. 501.

It has now been found that even at one and the same temperature, and that a temperature (150° C.) which is very far from a red heat, different substances emit very different kinds of heat, and that thus, in any space whatever, an extraordinarily large number of different wave-lengths are continually crossing each other. This manifold crossing is especially increased by the selective absorption which is met with at different surfaces.

Hence an eye which could discriminate the various wave-lengths of heat like the colours of light, would see all objects in the most different colours, even if they were not specially warmed.—Poggendorff's *Annalen*, September 1869.

ON THE LUMINOUS EFFECTS PRODUCED BY ELECTROSTATIC INDUCTION IN RAREFIED GASES.—LEYDEN JAR WITH GASEOUS COATINGS. NOTE BY M. F. P. LE ROUX.

I. In a previous communication I described a certain number of experiments which render evident the induction that takes place in the body of rarefied gases, in vessels formed of a continuous insulating material, and devoid of all metallic communication with the exterior. These effects are manifested by true currents which illuminate the gaseous masses in the body of which they are propagated.

The facts here treated of have interesting consequences in the way of explaining certain meteorological phenomena. They must play an important part in the luminous manifestations of the electricity of the globe to which is given the name of *polar auroras*; and the diffused part of the glows which constitute them, it seems to me, should be attributed to an electrostatical induction seated in the higher strata of the atmosphere, under the influence of the discharges of the aurora.

This same induction, operating in the rarefied strata of the atmosphere, seems to me to furnish the explanation of a remarkable circumstance which often accompanies the lustre of the lightning-discharge. When the lightning strikes, it produces an illumination which surrounds the perfectly serene regions of the sky, when there are any; the circumstances of this phenomenon do not appear to me to be capable of explanation by a phosphorescence of the atmosphere properly so called. It seems to me that we must rather perceive in it the manifestation of the return shock which must take place in the higher regions of the atmosphere at the moment when, through the effect of the discharge which constitutes the lightning, the clouds revert to their neutral condition.

As to the *heat-lightning*, so called, which is observed in a clear sky at a certain height above the horizon, there is no doubt that it is due to the same cause.

II. The electrostatical induction of rarefied gaseous masses appears to operate instantaneously across insulating envelopes; at least this is what seems to me to result from the working of the apparatus that I have constructed, in which the illumination is pro-

duced under the influence of a toothed disk of india-rubber previously electrified. We remark, in short, that the flash of the illumination increases with the velocity of the disk. This circumstance is but little favourable to the hypothesis according to which the influence would be exercised across dielectrics by a polarization of successive layers; it would be necessary in that case that the polarization should be instantaneous, and we cannot see in what the difference between insulating bodies and conductors would consist.

III. Tubes filled with rarefied gases and provided with metallic wires sealed at the ends like Geissler's tubes, but terminated externally by knobs to prevent the wires from acting like points, may be applied with advantage to demonstrate the movements of electricity to which the influence gives rise, especially those of the return shock. I have executed these experiments; but the credit of them is due to M. G. Govi, of Turin, who has very ingeniously employed this means of demonstration in the place of metallic conductors armed with pendulums, of the electroscopic frog, and of the other contrivances usually employed in this part of the study of electricity *. These luminous conductors have also been made use of by him to exhibit the phenomena of induction of different orders by interposing them in long metallic circuits.

IV. In the course of the experiments which I have had occasion to make with rarefied gases, I have remarked that the glass was charged by the intervention of gaseous conductors with the same facility as by means of metallic conductors. I have thus been led to construct a Leyden jar in which the metallic coatings are replaced by rarefied gas: it is composed of a closed primary tube enveloped by a second, to which it is fused; each of the tubes is provided with a platinum wire; a vacuum is created in them to the extent of about 3 millims. Such a system is charged with a Leyden jar of the same dimensions; the residues in it seem to be less abundant than in ordinary jars; but this question, in order to be fully solved, requires more numerous experiments.

In fine, rarefied gases behave precisely as metallic conductors. It is to be remarked that such a medium formed into a point acts just like a metal of the same shape, and manifests the same effects of tension, to such an extent that, in the glass vessels intended to contain gases with a view to the experiments here treated of, it is necessary to avoid all such tapering of the tubes as would give to the interior surface the form of an acute point. If this circumstance does happen, and the interior gas is strongly electrified, we often see the electricity strike out for itself a passage through the glass at that place; and if the glass be too thick, the electricity, in place of opening a direct path for itself, cracks off the little button of melted glass which generally terminates the tapering ends closed by the blowpipe.—*Comptes Rendus*, May 31, 1869.

* *Gazette officielle du Royaume d'Italie*, No. 49, 1865.

THE

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JOURNAL OF SCIENCE.

[FOURTH SERIES.]

DECEMBER 1869.

XLVII. *On the Motions of Camphor on the Surface of Water.* *By* Charles Tomlinson, *F.R.S.**

1. THE phenomena presented by the motions of camphor on water form a kind of scientific waif, which has at various times been claimed by certain scientific lords of the manor, quarrelled over, and then thrown aside. At one time it has wandered over the outer boundaries of science, occupying a sort of no-man's-land; at another it has been admitted into the best society, which latter position it may be said to occupy at the present time.

2. During the current year a remarkable memoir[1] has been *couronné* by the Royal Academy of Sciences of Belgium, and favourably reported on to the Academy[2] by that distinguished Belgian physicist M. Plateau. As the author has done me the honour of frequently referring to my labours, and was so good as to forward to me a copy of his memoir, I trust an account of it will not be considered out of place in the Philosophical Magazine.

3. But first it may be of advantage to give an account of the phenomena in question as briefly as is consistent with clearness. Some years ago I took considerable pains to read up all that had

* Communicated by the Author.

[1] *Sur la Tension superficielle des Liquides considérée au point de vue de certains mouvements observés à leur surface*, par G. Van der Mensbrugghe, Répétiteur à l'Université de Gand.

[2] *Bull. de l'Acad. Roy. des Sciences de Belgique*, 10th July, 1869.

been published on the subject; and it is chiefly from the account then given[3] that the following details are condensed.

4. In 1686 Dr. Heyde[4] noticed that when fragments of camphor placed on olive-oil are viewed under the microscope certain currents are observed, particles setting out, as it were, from a centre and returning to the same point.

5. In 1748 Romieu[5] first described the rapid gyrations of camphor on the surface of water: the motions are favoured by heat, and their cause is referred to electricity.

6. In 1773 Dr. Franklin[6], in his account of the effects of oil in stilling the waves, states that being about to show the experiment to Smeaton the engineer, on a small pond near his house, he was informed by Mr. Jessop, a pupil of Smeaton's, that in cleaning an oily cup in which some flies had been drowned, he threw the flies upon water, when they began to spin round very rapidly as if they were vigorously alive. "To show that this was not any effect of life renewed by the flies," says Franklin, "I imitated it by little bits of oiled chips and paper cut in the form of a comma of the size of a common fly, when the stream of repelling particles issuing from the point made the comma turn round the contrary way."

7. In 1785 Lichtenberg[7] notices that the camphor experiment succeeds best on warm water, or when the room is not very cold. On plunging a thermometer into water at 130° the motions suddenly ceased, in consequence of some alteration in the surface; or, as he says, the thermometer may not have been quite clean, so that the water became covered with a thin film. He refers the motions of the camphor to the varying attractions consequent on the constant change in form of the fragments brought about by solution and evaporation. He disproves the electrical theory of Romieu (5).

8. In 1787 Volta[8] examined the experiment with great care. He refers the motions to an effluvium which escapes from the camphor explosively after the manner of a firework, and produces motion by the force of reaction. Similar motions are produced by benzoic acid, salt of amber (succinic acid), and volatile concrete alkali (carbonate of ammonia). Salt of amber is particularly recommended, as it makes manifest to the eye the cause of the motions; for the fragment is evidently driven back from

[3] Experimental Essays, published in Weale's series, 1863. Essay I. On the Motions of Camphor on Water.

[4] *Centuria Observationum Medicarum.* Amsterdam, 1686. Obs. LVII.

[5] *Hist. de l'Acad. Roy. des Sciences de Paris*, 1762.

[6] Letter to Dr. Brownrigg, November 7, 1773. Posthumous Writings of Dr. B. Franklin, F.R.S. &c. London, 1819. Part IV. p. 268.

[7] *Delectus Opusculorum Medicorum*, edited by Frank. Ticini, 1787.

[8] Ibid.

the point where the effluvium is discharged most abundantly, covering the water and suffusing it with colour. It is further shown that when the water becomes impregnated with the camphor &c. the motions cease, that warm water and fine weather are favourable to the phenomena, that the purity of the water and of the containing vessel are necessary to success (indeed the success or failure of the experiment is a sort of indication of the purity of the water), that agitation of the water assists the experiment, and, lastly, that the gyrations take place on wine but not on spirits of wine, and not very well on olive-oil.

9. About the year 1794 Carradori[9] began to publish a number of papers and memoirs, *sull' attrazione di superficie*, in which he shows, by a great variety of ingenious experiments, that the surface of water exerts a remarkable attractive force on various bodies; and in 1800, referring to the motions of camphor, he says[10], "I prove that on this surface-attraction, and on no other cause, the motions of camphor depend." And again, "The mechanical force of the elastic vapour against the water has nothing to do with the phenomenon; it depends entirely on surface-attraction;" and in order to show that a non-volatile body will rotate, he repeats Franklin's experiment (6) on the gyration of bits of paper smeared with a fixed oil and thrown on the surface of water.

10. Several of Carradori's papers are in answer to the theory of B. Prevost[11], which attributes the motion of camphor and other volatile bodies to the formation of an atmosphere of elastic fluid round them, and to the impact of such fluid on the air. According to Prevost, a fragment of camphor of the size of a pea on a metallic disk four or five lines in diameter, and so placed on water, rotates.

11. Fourcroy[12], in reporting Prevost's paper, expressed his own opinion that these motions are due to the attraction of odorous matter both for air and for water, and their solution in one or both.

12. In 1797 Venturi[13] showed that a column of camphor fixed vertically in water wastes away chiefly at the junction of the air and the water. The oily matter of the camphor covers the surface and evaporates; and this explains the motion of camphor when free to move. This motion is the mechanical reaction which the oily substance, in spreading on the water, exerts on the camphor itself.

[9] *Opus. scelti di Milano*, vol. xx. *Giornale Fisico di Brugnatelli*, vol. vii. &c.

[10] *Giornale di Fisica &c. Pavia*, vol. i. p. 97. See also vols. iii., iv., viii., ix., and x.

[11] *Annales de Chimie*, vol. xxi. p. 254; vol. xxiv. p. 31.

[12] Ibid. [13] Ibid. vol. xxi. p. 262.

13. In 1800 Carradori[14] approves of this explanation and claims it as his own. The camphor owes its motion to the expansion of an oil drawn from it by the surface-attraction of the water. He combat's Prevost's theory (10), and denies that the camphor on a bit of cork or other substance floating on water has any motion. He insists on the energetic surface-attraction of water. Oils, whether fixed or volatile, have a strong adhesion or surface-attraction for water, but no cohesion or affinity of aggregation for it. White wax and hard suet, which have no odour and contain an oil that is not volatile, rotate on water. Oils, whether fixed or volatile, are more strongly attracted by the surface of the water than camphor is, and hence they arrest its motion. And not only so, but starch and other vegetable products and the juice of milky plants arrest the motions on account of the strong surface-attraction. Many odorous bodies that do not give out an oil to the surface of water have no motion.

14. In 1801 Prevost[15] denies Carradori's position (13), and further supports his own case by stating that minute fragments of camphor, benzoic acid, and dry musk rotate on clean dry mercury, and indeed on any clean dry surface. He has seen under the microscope minute fragments of camphor, too small for the unassisted eye, rotate on various kinds of support. Camphor will even rotate on small disks of mica placed on mercury.

15. In 1801 Biot[16] confirms some of Prevost's leading results, and gives the following experiment in support of his theory:—If a very small pointed cone of camphor be presented without contact to a thin film of water on a clean glass plate, it will repel the water and leave a dry space round it. Hence he concludes that camphor acts on water at a distance, and that its movements on water are due to the mechanical reaction produced on itself by the resistance which its vapour experiences in darting against the liquor which surrounds it, and that this emission of vapour is most abundant in the horizontal plane where the air and the water meet. The camphor-cone will also repel fragments of gold leaf floating in water without touching it or them.

16. In 1803 Carradori[17] replied to Prevost. It is curious to note the common feature of this and other scientific controversies, that one man cannot follow the reasoning or even repeat the experiments of his antagonist, so difficult does observation become when another man's results are looked at through the spectacles of one's own theory. Thus Carradori denies that a capsule of

[14] *Annales de Chimie*, vol. xxxvii. p. 38. [15] Ibid. vol. xl. p. 3.

[16] *Bulletin des Sciences par la Société Philomatique*, No. 54, p. 42.

[17] *Annales de Chimie*, vol. xlviii. p. 197.

ether suspended over water containing bits of gold leaf repels them by its vapour acting at a distance. He denies that camphor on a raft floating on water rotates; while Prevost, on his part, knows nothing of surface-attraction, or of the oil that is said to issue from camphor in contact with water, and which is said to produce rotation by its reaction on the fragment. He has looked in vain for such oil, and believes it exists only in the imagination of the Italian physicist. Carradori replies, "What wonder is it that camphor should cover the water with an oily film, since camphor is itself a very volatile concrete oil?" He insists on surface-attraction, and cites this ingenious experiment:—A bottle 2 inches in diameter with a neck only 3 lines in diameter was filled with water; fragments of camphor thrown into the narrow neck did not rotate for want of a sufficient expanse of surface-attraction. Enough water was drawn out by means of a straw so as to lower the surface to the wide part of the bottle, when the camphor rotated briskly on the larger surface. Here, again, the two observers are at variance; for Prevost, in his former paper (14), says that camphor will move in capillary tubes previously cleaned by drawing threads through them, and that lively motions may be seen in them with the aid of a magnifying-glass.

17. In 1812 we meet with Carradori again[18]. He describes some experiments, based on an observation by Accum, that phosphorus rotates on the surface of mercury. He gives this as a further illustration of the attraction of surface, the phosphorus covering the mercury with a subtle varnish which gradually arrests the motion; but it may be renewed by filtering the mercury. Phosphorus was also found to rotate on the surface of tepid water.

18. In 1820 Serullas[19] describes the motions of alloys of potassium, sodium, &c. on a shallow surface of water 1 or 2 lines deep resting on mercury. Small fragments of the alloy of potassium and antimony rotated, disengaging hydrogen, especially from one point: each fragment described a circular path in the opposite direction to the point of greatest liberation of the gas. An alloy of potassium and bismuth rotates on the surface of mercury. An alloy of potassium with lead or tin does the same; but if water be added the motions are more rapid. The smaller the fragments the more rapid the motions: "on les voit voltiger avec une étonnante vivacité: on dirait des mouchons retenus dans les piéges, faisant des efforts pour s'en délivrer"[20]. Alloys

[18] *Giornale di Fisica &c. di Brugnatelli,* vol. iii. pp. 261, 373; vol. iv. p. 297.

[19] *Journal de Physique,* vol. xci. p. 172.

[20] Prevost also says of the motions of camphor on mercury, "on eût dit les y voir voltiger," for they scarcely touched the mercury.

of sodium with most of the metals also rotate on mercury, or on a thin plate of water on mercury.

19. In 1825 the brothers Weber[21], in noticing Franklin's experiment (6), reiterate the fact that a downy feather smeared with oil rotates on water, and express their opinion that the motions of camphor and of various other bodies on water still remain to be accounted for by a satisfactory theory.

20. In 1833 Matteucci[22] states that raspings of cork steeped in ether rotate on the surface of water, and continue to do so as long as the surface is supplied with ether, as by conducting a thread from the ether bottle to the surface. His conclusion is that it is to the currents of volatile substances that the motions are due.

21. In 1841 Dutrochet[23] described the following experiment:—If cork be steeped in a solution of caustic alkali and dried and then be placed on water, the solution is projected strongly from the cork, and this moves in the opposite direction. "This motion of the cork is evidently the effect of recoil produced by the repulsion which the solid alkali contained in the cork exerts on its own solution. It is very probable that this repulsion is electrical, and arises from the fact that the solid body dissolved has a similar electricity to that of the solution. However this may be, the fact of the reciprocal repulsion of the soluble body and of the aqueous solution is certain, and it is to this repulsion that we may attribute the motion that takes place at the surface of water of all floating bodies that dissolve in it. This occurs not only in the case of alkalies, acids, and salts, but in gum resins, such as opium, aloes, &c."[24].

22. In 1841 Messrs. Joly and Boisgiraud[25] bring before the

[21] *Wellenlehre.* Leipzig, 1825.

[22] *Ann. de Chim. et de Phys.* vol. liii. p. 216.

[23] *Comptes Rendus*, vol. xii. p. 2.

[24] This experiment is evidently based on Prevost's experiments (note [15]), intended to show that almost all liquids are each susceptible of repelling all others or of being repelled by them; that is, if a liquid be made to cover a glass plate, and a drop of another liquid properly selected be placed on the film, the latter will be driven away and the second will occupy its place. Thus

Ether	repels	Alcohol.
Alcohol	,,	Essential oil of peppermint.
Oil of peppermint	,,	Oil of bergamot.
Oil of bergamot	,,	Oil of origanum.
Oil of origanum	,,	Oil of savory.
Oil of savory	,,	Fixed oils.

So also pure water repels many solutions of salts. A solution of alum repels one of vitriol; this repels sodic sulphate; this potassic nitrate; this sodic chloride, and so on.

[25] *Comptes Rendus* for 1841, p. 690, which contains a Report on the Memoir.

Academy of Sciences a memoir which clashes a good deal with Dutrochet's (21); and the noise is heard at intervals during this and the first half of the following year. The authors do not seem to have added much to the subject in hand. They found that thin slices of cloves, pepper, orange-peel, &c. rotated on water, and that naphthalin, though motionless on the surface of water, rotated briskly on that of mercury. The advantage of working with mercury is that it renders visible effects which are not seen on the surface of water.

23. Although Dutrochet's researches (21) occupy nearly seventy pages of the *Comptes Rendus* between the 4th of January and the 5th of April, 1841, he felt that he had published them with too much precipitation, and accordingly retired for awhile in order to reconsider the whole subject. This led to the publication of a separate work, in two parts[26], in which not only the motions of camphor, but a vast number of other interesting facts are traced to the influence of a force residing on the surface of liquids, and hence named *epipolic* (ἐπιπολὴ, *surface*). He does not admit, and probably did not see, that this is nothing more than another name for Carradori's attraction of surface (9), (13), (16), (17); for he does not seem to have been master of the Italian language, in which Carradori's earlier memoirs are printed, and that at a time when the noise of conquest would scarcely allow the voice of science to extend so far as from Italy to France, unless it were unusually loud, as when Galvani and Volta spoke for her. In the early part of his work Dutrochet says that "when a bit of camphor is placed on the surface of water, there forms around it a portion of camphorated water, which immediately becomes endowed with a rapid centrifugal extension due to the development of the epipolic force. The morsel of camphor, surrounded by camphorated water incessantly renewed and incessantly projected circularly on the surface of the surrounding water by a kind of intermittent explosion, must necessarily partake by reaction of the motions of the liquid which surrounds it, and receives from it those motions of progression which we see it execute on the surface of the water. Such is, in short, the cause of this phenomenon"[27]. In the second part of his treatise he says:—"The motion of camphor on water is an effect of reaction produced by heat-repelling epipolic currents, which are formed near the small fragment of this volatile substance, especially near its points or angular parts" (part ii. p. 159). "Everything concurs to prove that these epipolic currents, produced on water by a morsel of camphor placed on the surface of that liquid, are due to the local heat developed on such surface by the vapour of

[26] *Recherches Physiques sur la Force Epipolique*, part i. 1842; part ii. March 1843.

[27] Ibid. part i. p. 74.

the morsel of camphor, and probably also by its immediate contact"[28].

24. In 1861–62 I was led by the phenomena of cohesion-figures to pay some attention to the motions of camphor &c. on water[29]. It was evident that Carradori's attraction of surface exerted a powerful influence on the phenomena, since a globule of creosote, carbolic acid, &c. on the surface would sail about and exhibit the most lively motions and even be torn to pieces and disappear in the course of some seconds, while below the surface a drop would remain as a globule unchanged for hours or even days. So also a drop of a solution of camphor in benzole &c. would move over the surface, darting out waving tongues and so disappearing. But phenomena of this kind seemed to be simple effects of adhesion of surface, tending to overcome the cohesion of the drop by spreading it out into the form of a film; and the various amounts of resistance offered by different liquids led to such different resultant phenomena as those of cohesion-figures, and the various motions of camphor and other bodies. But in the case of camphor and other solid bodies, not only was a film detached from its surface by the adhesion of the water, but the reaction of this film on the fragment seemed to be a sufficient force to account for its gyrations. It is true that in the case of camphor the film is not visible, but in many other cases this objection does not apply. Oil of aniseed, for example, solidified by cold, gyrates like camphor, only more slowly, with the advantage of leaving a filmy trail on the surface. A fragment of this oil on water, apparently performing the whole of its work under the eye of the observer, seemed to give irresistible proof of the truth of the theory, viz. that the adhesion of the water detaches a film from the solid, which film in the act of spreading on the surface, produces motion by reaction. If the film remain on the surface the motion ceases; but if it be rapidly disposed of by evaporation and solution, the motion may continue so long as the fragment lasts. If proper arrangements be made, motions which admirably represent the phenomena may be kept up for days together. For example, if a three- or four-sided stick of camphor held in forceps be made to dip just below the surface of clean water previously dusted with a very thin coating of lycopodium-powder, a film is detached from each side of the camphor the moment it touches the water; there is instant repulsion of the powder as by a flash; then a momentary pause, during which the film is disposed of by evaporation and solution; another film is detached in like manner, and the solution of camphor from each film, corresponding with each side of the stick, travelling on, or rather

[28] *Recherches Physiques sur la Force Epipolique*, part ii. p. 160.

[29] See note [3].

being propelled on by successive films to the curved surface of the glass, divides and curls round in two opposite directions, thus producing a pair of wheels for each face of the camphor, which the lycopodium renders distinctly visible. I have allowed this action to go on during sixty hours with no other interruption than having to lower the stick two or three times when a portion had been cut off by the sawing action of the surface-water.

Now this process, like a machine in motion which goes on so long as it is wound up, fails unless free course be given to the evaporation of the camphor-film. The experiment cannot be conducted in a large bottle. The camphor has been made to dip into the water contained in a clean bottle: at first there were faint indications of a current; but these soon ceased. After many hours some of the water was poured from the bottle into an open vessel; and the moment the camphor was lowered into it, the currents set in with much of their accustomed vigour. The experiment also fails if the lycopodium dust be laid on too thickly; a very faint shower from a muslin bag is sufficient for the purpose. The motions are more vigorous on a bright clear day than on a dull cloudy one, more active in summer than in winter.

25. That this experiment depended on the constant formation and evaporation of a film of camphor seemed to be evident from the perfect way in which it could be imitated by means of ether. At the end of a narrow tube a bit of sponge was tied, and the tube filled with ether was supported vertically about an inch above the surface of water previously dusted with lycopodium; a very perfect, sharply cut, well-defined disk of ether is formed on the surface of the water by the condensation of the vapour pouring down from the sponge. The disk does not increase in diameter, but the excess of ether pours off from it and proceeds radially to the surface of the glass, where each branch curls round in two opposite directions, throwing the powder into pairs of wheels precisely as in the case of the camphor current (24).

26. Another phenomenon, which I named "camphor pulsations," seemed also to illustrate the view I had taken of these motions. A stick of camphor with a square base is lowered so as to touch the bottom of a shallow glass vessel 6 or 7 inches in diameter, containing a little water, not more than about two ounces. As soon as the camphor touches the water the whole surface becomes agitated with rapid pulsations, at least 250 per minute. As the water soon becomes saturated, the pulsations gradually diminish to 60 or 80 per minute, and they may even sink down to 8 or 10 per minute.

According to my explanation, as soon as the camphor is low-

ered to the bottom of the vessel, the water rises by capillary attraction some way up the stick and detaches a portion of its substance, which is then spread out as a film by surface adhesion and disposed of by solution and evaporation. As the film is being detached, it repels the water from the camphor and produces a depression of surface all round the stick; the water recovers itself, capillarity again comes into play, another film is detached, and matters proceed as before—the result being a series of pulsations or waves which rise up so that at length their crest may be one, two, or three tenths of an inch above the general surface of the water. The variations in height are marked by a series of curved grooves or ripple-lines on the sides of the camphor, which gradually exchanges its dull translucent appearance for a bright transparent one, showing that the water has penetrated it. In the meantime an incision is made in the camphor, which goes on increasing as successive films are detached, until the stick is cut through and the submerged portion rises to the surface and commences a series of gyrations on its own account.

27. As, in the case of small fragments of camphor rotating on the surface of water, the motions are stopped if the surface be touched with a fatty oil, so these pulsations are immediately arrested if the water be touched with a drop of any substance which forms a film and arrests evaporation. The point of a pin dipped into olive-oil and brought into contact with the water at once stopped the lycopodium currents (25); a second contact stopped the pulsations (26). So also if a body be added to the water that satisfies its adhesion so as to stop the solution of the camphor, the pulsations are arrested. Thus a drop of oil of camphor stops the pulsations by depriving the water of the power of dissolving camphor; a drop of olive-oil stops the pulsations by preventing evaporation; but a drop of oil of bitter almonds, which speedily evaporates, allows the pulsations to go on after a slight interruption. Turpentine and bodies that leave a permanent film stop the pulsations; but ether, alcohol, benzole, bisulphide of carbon, caustic potash, and sal-ammoniac allow them to go on. A bit of sponge tied to the end of a glass rod, dipped into ether and held near the camphor, will hold up the wave of water against the camphor for some time. A drop of benzole does not stop the pulsations; but it makes them less rapid. The pulsations go on in a solution of caustic potash and in one of sal-ammoniac. The pulsations and rotations of camphor are not arrested by the addition of acids to the water, including butyric acid. Camphor even rotates on the surface of acetic acid.

28. In 1863 I obtained a result[30] which seemed to place the essential oils in a new light with respect to the surface of water. It

[30] Phil. Mag. September 1863.

was shown in my original essay that essential oils did not permanently arrest the motions of camphor, but only so long as they remained in the form of films on its surface. When these had evaporated without leaving any residue or oxidized deposit, the motions set in as before. But I now found that if the oils were freed from oxidized products by being distilled in contact with a bit of sodium or caustic potash, they did not arrest the motions of the camphor at all. The fragments skated through them and cut them up in all directions. The oils had so far improved in cohesive force that they no longer formed films, but lenticular masses with rounded edges. From ten to twenty drops of an oil might thus be deposited on the water without interfering in any way with the gyrations. Fragments of benzoic acid, obtained by exposing oil of bitter almonds, or of *Laurus cerasi*, to the air for some time, were singularly active below, in, and among the oil. This showed that there was little or no adhesion of the oils to the surface of the water; so that the fragments were as free to move as if the oil were not present.

29. It was not until after reading Professor Van der Mensbrugghe's memoir (note [1]) that I attempted to repeat the experiment of camphor on a raft on the surface of water (10). It was evident to me that if this were a true result, it would be fatal to the reaction theory—although Prevost (14) and Biot (15) insist on the force of the experiment, and explain it on the principle of reaction on the air, while Carradori (13) is equally energetic in denying the possibility of the experiment unless there is reaction on the surface of the water. I placed camphor on a tinfoil raft and also on cork, and never obtained any motion unless the water wetted the camphor, or had some direct communication with it. Professor Mensbrugghe suggests that my rafts and their cargo of camphor were too heavy. I now see that this was the case, and that the cork, from being too thick, was too high out of the water. I formed a raft of a small square of mica, placed on it a bit of camphor about the size of a small pea, took up the raft on the point of a penknife, and so launched it upon the surface of 6 ounces of water contained in a very clean cohesion-figure glass $3\frac{1}{2}$ inches in diameter. Before the raft had touched the water, a visible shudder passed over its surface, showing the action of camphor at a distance, as in Biot's experiment (15). No sooner was the raft fairly launched than it began to sail about, and continued to do so with gradually slackening effort during a whole week. The advantage of using mica is that its surface is almost *à fleur d'eau*, and it sails about without allowing the camphor to be disturbed or to become wet.

30. The principle upon which the new theory is based is that

of the surface tension of liquids. The researches of Segner[31] in 1751, and of Dr. Thomas Young[32] in 1806, rendered it very probable that there existed a contractile force or tension at the surface of liquids. The labours of Henry[33], Lamarle[34], Dupré de Rennes[35], Van der Mensbrugghe[36], and others have converted this probability into a certainty; so that the existence of such a force (which is a more perfect definition of Carradori's *attraction of surface* (9), and of Dutrochet's *epipolic force* (23)) is not only capable of proof, but can also be expressed numerically for different liquids at a given temperature. As this force cannot be said to be yet recognized in our Manuals of Physics, perhaps I may be excused for quoting the following lines from one of the few books, intended for the use of the student, in which it is noticed:—

"Every liquid possesses a certain amount of *tenacity* or *direct cohesion*, whereby its parts resist separation by being directly torn asunder. This cohesion has been proved to be the result, in whole or in part, of an attractive force between the particles of the liquid, which acts at appreciable though exceedingly small distances; in consequence of which there exists at the external surface of every liquid mass a layer or film of liquid of unknown but exceedingly small thickness, which is of somewhat less density than the internal mass of liquid, and consequently in a state of tension. This superficial tension is the force which sustains a hanging drop; and its amount may be computed from the weight and dimensions of the largest drop of the liquid which can hang. It causes the surface of every isolated mass of liquid (such as a falling drop), or cavity in a mass of liquid (such as an air-bubble), to contract to the smallest possible dimensions, and consequently to assume the figure of a sphere. It also causes the surface of every isolated jet of liquid to tend to assume a form of circular section, or to oscillate about such a form. It modifies the form of the surface of every mass of liquid by rounding more or less the corners, which would otherwise be angular. Cohesion also exists to a greater or less degree between liquids and solids; and the combined effects of this force and of the superficial tension due to the cohesion of the liquids themselves, constitute what are known as phenomena of *capillary attraction*. It is by reason of this tendency of the external film of a liquid mass to assume a definite figure, viz. the sphere, that, in defining the word 'liquid,' non-resistance to change of figure

[31] *De Figuris Superficierum fluidarum comment.* Götting. 1751.
[32] Phil. Trans. 1805. Essay on the Cohesion of Fluids, p. 65.
[33] Phil. Mag. 1845.
[34] *Mém. de l'Acad. Roy. de Belgique,* 1864.
[35] *Ann. de Chim. et de Phys.* Ser. 4, vols. vii., ix., &c.
Bull. de l'Acad. Roy. de Belgique, vols. xxii., xxiii.

has been predicated of the interior parts of a liquid body only and not of the whole mass"[37].

31. In order to produce distinct experimental results on surface-tension, Professor Van der Mensbrugghe had to devise a method by which one portion of a tensile liquid surface could be separated from another portion of the same surface, so as to show variations in tension between the two portions. For this purpose filaments of a silkworm's cocoon were cut into lengths of about 12 centimetres, and, ten or fifteen of these being laid parallel, were tied at the two extremities. The bundle thus formed was bent into an irregular circle, washed in alcohol and then in distilled water, and flattened between the leaves of a book. The bundle was now taken up by means of a clean glass rod, and placed on the surface of water in such a way as to be exactly in contact with it without being below the level.

32. Let the two liquids be distilled water (whose surface-tension is equal to 7·3) and ether (of which the tension is 1·88). The water is contained in a large capsule, and a drop of ether is held above that portion of the surface limited by the coil of filaments; this immediately undergoes lively trepidations, and tends to assume the circular form, evidently because the vapour of ether diminishes the tension of the subjacent portion of surface within the silken boundary, and this, in its turn, yields to the superior traction of the portion external to it. The moment the drop of ether touches the surface within the flexible contour, the silk expands into a circular form; but it as quickly contracts, since the evaporation of the ether cools the surface and so restores its contractile force. When, on the other hand, the ether is deposited outside the silken boundary, this immediately becomes reduced in size, but expands again as the cold produced by evaporation augments the contractile force of the exterior portion.

33. In this way may be explained the observation of Prevost (14), that if a bit of camphor be held near the surface of water that has been dusted with lycopodium, the powder is repelled towards the edge of the vessel; or, as in Biot's experiment (15), if camphor be brought near a thin layer of water, this opens and leaves a dry space on the support just under the camphor. In such cases the water locally dissolves a small quantity of the vapour of camphor, and thereby has its tension locally reduced, while the contractile force of the other parts of the surface is free to act.

34. The rotations of camphor on the surface of a liquid, and similar phenomena, are included in the following general propo-

[37] Nichols's 'Cyclopædia of the Physical Sciences,' 2nd edit. 1860. Art. "Liquid."

sition :—When on the surface of a liquid, A, we deposit a small fragment of a solid, B, which is more or less soluble in A, or detaches from its surface matter that is so, the equilibrium of the superficial layer of A is disturbed. If the solution take place equally all round the fragment, this does not move; if unequally in different azimuths, the fragment displays sudden movements of translation and rotation.

35. In order to show the action of camphor in diminishing the surface-tension of water, flexible filaments were taken, 30 or 40 centims. in length. On scraping a few fragments of camphor upon the space defined by the filaments, these were quickly thrown into the form of a perfect circle. The camphor produced great diminution in the contractile force of the water, reducing it to 4·5; and as this diminution takes place unequally round each fragment, this must necessarily rotate. If the camphor be placed outside the ring, the filaments immediately contract.

36. By repeatedly adding fragments of camphor to the water, this became reduced in tension to 4·5, and the camphor no longer rotated. Or if the surface be touched with the finger, the tension is reduced to 4·75 in consequence of a greasy film being transferred to the water. A similar effect is produced by an unclean vessel, or the presence of smoke, or of the vapours of essential oils &c. in the air of the room.

37. The various bodies that rotate on water act like camphor in locally diminishing its surface-tension. The reason why the motions are not in general observed on the surface of oils, spirit, &c. is, that their surface-tension is feeble, although their adhesion to the camphor &c. is sufficiently energetic to dissolve it.

38. There are many circumstances which render this theory more acceptable than the recoil theory, which has so long found favour in accounting for these motions. For example, in one of the experiments described in my essay (note [3]), a well-shaped lens of water with a well-defined rounded edge was formed on a glass plate, and also on the surface of clean, pure mercury, and on this lens minute fragments of camphor were set spinning. I observed that the fragments would often pass over the edge and rotate in a nearly vertical tangent plane, and then go back again to the upper surface of the lens. A similar effect was also noticed with phosphorus on the surface of mercury. I could not understand by what influence the fragments recovered their position from a nearly vertical to a horizontal plane. The surface-tension theory makes it clear. Another difficulty was that the rotations of bits of paper smeared with oil are very rapid on the surface of water, notwithstanding the friction; flakes of camphor, formed by exposing oil of camphor to the air, or flakes of benzoic acid, formed by a similar exposure of oil of bitter almonds, move with even

greater rapidity; indeed their gyrations are sometimes so rapid as to make the fragment appear hazy. This also occurs when the ether-sponge is held over the rotating camphor. Flakes of solid acetic acid are amazingly active on water; while the needles of solid carbolic acid have a peculiar rapid jerking kind of motion, not consistent with the reaction of the solution on the fragment. Then, again, the sharply defined character of the perfectly circular disk of ether formed by holding the ether-sponge over the surface of the water (25) seemed to point to the action of a force acting equally around and exterior to the disk.

39. With respect to the rotations on the surface of mercury in which the camphor &c. are not soluble, the theory is not quite so clear. But I gather from the memoir that the rotations are due to variations in surface-tension consequent on the adhesion of the camphor. This must be very slight; for Prevost says (note [20]) the fragments seemed scarcely to touch the surface. I also do not see how Prevost's experiment (14) on the motions of camphor on solid plane surfaces is to be accounted for on this theory. It is very desirable to repeat this experiment; and I hope some of our microscopists will do so. I also do not see how the case of pure or recently distilled essential oils, occupying the surface of the water without interfering with the motions of the camphor (28), is met by the theory, unless it can be said that the oil is bound up, as it were, by its own surface-tension, so as not to interfere with the surface-tension of the water. If this condition be admitted, the fragments are as free to move as if the oil were not present. Although the fragments pass through and cut up the oil, the latter does not lose its lenticular form, so that its tension is probably not diminished by the presence of the camphor.

40. There are a large number of facts contained in, or suggested by this memoir (such as those relating to the action of vapours and films on the surface of water), which may perhaps call for a separate notice. But as far as the motions of camphor &c. on the surface of water are concerned, I am bound to admit (notwithstanding 39) that this curious and suggestive problem, which has occupied so many scientific minds during nearly two centuries, has at length received a satisfactory solution. And this, like every true scientific work, has absorbed a vast number of phenomena which apparently had little or no mutual connexion. During these two centuries many labourers have been working in the same field, tilling a difficult soil, which to the most diligent culture never yields a harvest, but only now and then a few grains, for which, it may be, the proper granary is not known, until at length the master comes and collects the

grain from the various labourers into the proper storehouse which Nature herself condescends to point out to him. Such I believe to have been done by the Belgian whose work I have surveyed with so much pleasure and profit. All honour to him!

Highgate, N., Nov. 13, 1869.

XLVIII. *Microscopical Investigation of thin polished Laminæ of the Knyahynia Meteorite. By* Professor A. Kenngott, *of Zurich**.

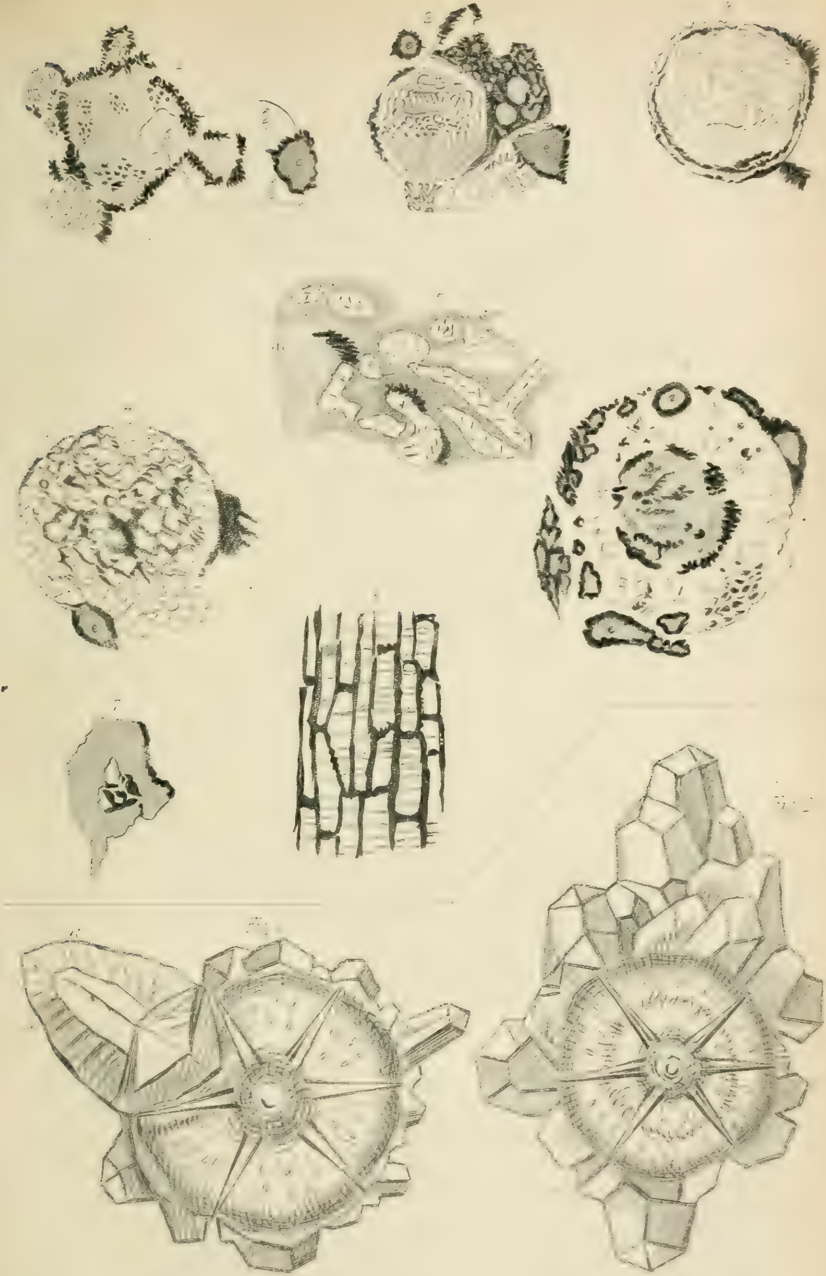
[With a Plate.]

The general tint of these laminæ is grey, spotted with yellow; they are semitransparent, with the exception of some opaque or dark-yellow spots. Incident light shows not unfrequently minute spots of metallic lustre. The whole appears fine-grained to the unassisted eye, and spheroidally grained ("oolitic," to use a somewhat inadequate term) under a magnifying-power of two to four. The granules are grey, some of them more or less angular; the yellow tints appear only in irregular spots, not being proper to any distinct component. Opaque substances are irregularly interspersed, in some cases marking the outlines of isolated granules. The spherical granules pass gradually into angular forms with rounded edges; and some of them lose their rounded form under strong magnifying-powers. Rounded and distinct sections appear scarce under a thirtyfold magnifying-power, which has proved the best for examining the structure in its totality.

Besides the metallic and opaque particles, two crystalline mineral species are discernible; one of them is colourless and transparent, the other grey and translucent; both are bi-refractive, and show various polarization colours, not separated from each other by distinct limits. Some spherules consist essentially of one or the other of these minerals; in others their outlines have become indistinct. The opaque substances are subordinate, nor have they any influence on the structure, being merely interposed among the rounded or angular granules.

The structure of the Knyahynia meteorite (the relative size being left out of consideration) reminds one of the *globular diorite* of Corsica, and may therefore be supposed to be rather the result of a process of crystallization within its own substance than an aggregation of separately formed corpuscles. The opaque components are light-grey metallic iron, greyish-yellow magnetic iron-pyrites (Haidinger's "troilite"), and a black substance.

* From a letter to Chevalier W. de Haidinger, read to the Imperial Academy of Vienna, May 13, 1869. Translated and communicated by Count Marschall, F.C.G.S. &c.



These three components may be best discerned by the microscopical examination of the laminæ under *incident* light. If the light from above is stopped, they all appear black by transmitted light. If light from above is admitted, only the black substance *seems* to be opaque, the iron appearing dark-grey and translucent, and the pyrites blackish yellow and faintly diaphanous by the effect of reflected light. This optical illusion could not be left unnoticed; as, besides the frequent grey and translucent minerals, another dark-yellow faintly diaphanous substance is visible at two places of the lamina.

The grey and the uncoloured silicates are differently affected by hydrochloric acid; and it may be inferred from this different action, and from the crystalline structure, that the first is *pyroxenic* (probably *enstatite*), and the other *peridotic*. The grey silicate, if polished, shows stripes, indicative of lamellar structure; the hyaline one shows merely fissures. Both appear in angular and rounded granules.

Plate III. fig. 1 shows the section of a granule nearly everywhere surrounded by irregular angular granules of the black opaque mineral. Its diameter is 0·48 to 0·64 millim.; it is imperfectly round, and is surrounded by transparent particles, except at four places, where it is in contact with small particles of the black mineral. It shows distinct stripes, also appearing in the small granule on the right, the other three showing merely irregular minute stripes. An extremely delicate transparent substance interposed between the grey, partly parallel, partly divergent stripes, makes them perceptible. Some few isolated black points lie within the round granule. Further to the right (at *e*) is metallic iron, with a black opaque substance around it; and a yellowish tint, equally affecting the grey and the hyaline silicate (indicated by the outline and the letter *g*), extends into the rounded granule. The tinging substance is oxyhydrate of iron. The black particles lying isolated within the granule and around it have undoubtedly been expelled outward by the progress of crystallization. The structure just described becomes more and more indistinct as the magnifying-power is increased, and resolves itself into a mere aggregation of grey and hyaline particles when the power is $=900$.

Fig. 2 represents another object, 0·5 to 0·6 millim. in diameter, of which (perhaps rather fortuitously) the greater half offers the form of a hexagon. This granule is essentially composed of the grey mineral, showing linear formation only in its lower portion—its upper half showing irregular, light-coloured, rounded spots with darker margins, reminding one of granular texture. The whole is framed in by a light-coloured border with isolated fissures, which is distinctly limited by an aggregation of

the black opaque substance in minute granules. On the right (at *e*) is metallic iron bordered with black, and on the left, above, another minute particle of iron. The dark granular substance outside and above the figure is granular magnetic pyrites (*troilite*) connected and framed by black opaque substance. The distinctly linear portion of the granule touches a small portion of striped grey substance below, which separates it from the iron (*e*), and from a diaphanous fissured granule. A number of particles of the black opaque substance become visible in the interior of the granule under a magnifying-power of 120 to 330.

The third object (fig. 3) is a round granule of the grey mineral, 0·7 millim. in diameter, nearly circular, rather distinctly limited by a double row of minute opaque black granules accumulated laterally into two black spots. The whole surface appears made up of white and grey under a magnifying-power of 75 to 120, and spotted or speckled under a higher power. Some large fissures run irregularly through the whole. The double border of black granules is worthy of particular notice. A magnifying-power of 450 and more shows the whole to be interspersed with extremely minute yellow granules, quite different from the irregular yellow tints of some single places more or less spread over the whole polished surface.

The grey mineral constitutes essentially the round or rounded granules figured in figs. 1, 2, and 3, besides many others, larger and smaller, and more or less varied. All of them prove this mineral to possess a certain degree of crystalline structure, as it is observable in enstatite and diallage, and manifested by linear stripes on the sections under certain aspects. An oblong round granule of 0·8 to 1·2 millim. shows several groups of parallel stripes, one near the other, as would an aggregation of a number of individuals. Another granule, 0·6 millim. in diameter, presents very dark stripes together with lighter ones. The black opaque granules along or near the margin are rarely wanting.

Other granules consist of a compound of a transparent and of a translucent mineral substance. Fig. 4 is a large round granule 1·5 millim. in diameter, showing a crystalline granular aggregation of the transparent silicate, with irregularly angular or rounded granules cemented together by the dark-grey silicate. Some few black granules appear locally, accumulated here and there along the margin of the outline. A small portion of metallic iron, bordered with a black substance, appears at *e*; and at another place is a dark spot of magnetic pyrites, smaller than that in fig. 2, and likewise bordered with black substance. Another granule, 0·8 millim. in diameter, shows within a light-coloured border (about 0·08 millim. in breadth) an aggregation similar to that in fig. 4, only the transparent granules are rela-

tively larger, and the grey substance is of somewhat lighter tint. The margin is exclusively formed by the transparent fissured mineral. The somewhat sinuated outline of the whole granule (or rather of its section) is marked in some places by black granules. A rounded section, 0·6 millim. in diameter, is merely a crystalline granular aggregation of transparent silicate, with many black opaque granules more approximated towards the margin than in the central region. Wherever the rounded granules appear less distinctly, the granular aggregations of the transparent silicate are irregularly associated with the grey one, whose stripes are then no longer perceptible. Where the grey silicate prevails (as in the portion, 1 millim. in breadth, shown in fig. 5), the stripes become more distinct and appear either parallel or divergent.

The specimens hitherto described prove both silicates to have crystallized *simultaneously*—one or the other of them, according to circumstances, having accumulated around certain centres in a spherical form, thus imparting to the meteorite, as a whole, a somewhat oolitic aspect. An alternation of substances within one and the same granule, as it occurs in globular diorite, is seen in the section of a granule 1·5 millim. in diameter. In its interior the grey mineral with irregular fine stripes is associated and partly framed with the black opaque substance (see fig. 6). Around this central portion is a granular aggregation of the transparent fissured silicate, locally interspersed with granules of the black opaque substance and of metallic iron. The outer border is marked by irregular particles of iron bordered with black substance. Small yellow granules of magnetic pyrites, associated with black substance (as in fig. 2), appear on the left side.

The grey mineral is likewise the essential component of another rounded granule, 0·36 millim. in diameter, some few linear individuals appearing more conspicuously. A broad marginal zone includes some black granules. The whole granule is surrounded with portions of the three opaque minerals, comparatively more extensive than those in fig. 6, and themselves parts of a more extensive zone of granular crystalloids of the transparent mineral, whose intervals are filled up with amorphous particles of the grey mineral. This zone gradually vanishes into the general aggregation.

A third granule, 1 millim. in diameter, shows likewise a grey nucleus and a surrounding transparent zone, both including abundant particles of black substance and magnetic pyrites.

Fig. 8 is a portion of the transparent mineral, 1 millim. in length and 2 millims. in breadth, whose appearance and optical condition are those of *one single individual*, interwoven with another dark greenish brown, faintly pellucid mineral, and itself ex-

hibiting a great number of fissures in nearly equal directions. A similar but by far smaller portion appears in a rounded section, 0·6 millim. in breadth, occupying one-half of the whole diameter, and bordered on both sides by granular aggregations of the transparent mineral.

The metallic iron, like the two other opaque minerals, generally appears interspersed in proportionally minute particles. In some few cases (see fig. 7) particles of iron, of 0·6 to 0·8 millim., include granules of the transparent silicate, with some few black granules in its interior, and others at the external margin of the central granule and of the iron.

Small fragments acted on by the blowpipe-flame are locally covered with a black glossy enamel. The grey powder of the meteorite, brought into contact with curcuma-paper moistened by distilled water, offers a distinct and sometimes intense alkaline reaction; it is partly soluble in hydrochloric acid, emitting sulphuretted hydrogen and leaving gelatinous silica.

XLIX. *The Parallelogram of Forces.*

By William Henry Preece, *Assoc. Inst. C.E. &c.**

It is said that there are twenty-seven known proofs of the parallelogram of forces. Any attempt to add to this number appears to be a needless undertaking; but the proofs usually inserted in elementary works are generally so laboured, that beginners rarely succeed in mastering them fully in their first journey through statics. Indeed it appears to me that the proof that the resultant is represented in magnitude as well as in direction by the diagonal, as usually given, is defective; for we are required to draw a line equal to an unknown quantity, and then to show that another line is equal to this line without obtaining the unknown quantity.

I have therefore ventured to arrange another proof based upon the principle of *couples*, which not only attempts to remove this defect, but to free the usual proofs from the necessity of subdividing the proposition into the two cases of commensurable and incommensurable forces—a veritable *pons asinorum* to all students.

Definitions.

(1) A *couple* is a system of two equal forces acting in dissimilar directions in parallel lines.

(2) The *arm of a couple* is the perpendicular distance between the lines of direction of the two forces.

* Communicated by the Author.

(3) The *moment of a couple* is the product of the magnitude of either force into the arm of the couple. (It is the numerical measure of its importance.)

Axioms.

(1) Any system of forces may be replaced by their resultant.

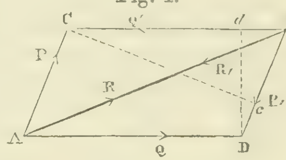
(2) Two equal and opposite forces acting on different points of a rigid body, so as to balance each other, are upon the same straight line.

(3) Two equal and opposite couples acting at the same point of the same rigid body, balance each other.

(This is a Cor. to Definition 3; for the two couples have the same moments, but of different signs.)

1. *Let the two forces* P, Q *act upon the point* A; *it is required to find the direction of their resultant.*

Fig. 1.



Take A C, A D respectively equal in magnitude and direction to the forces P, Q. Through C draw C B parallel to A D, and through D draw D B parallel to A C, meeting C B in B. Join A B. Then A C B D is a parallelogram, and A B is its diagonal.

At B, rigidly connected with A, apply a force P_1 equal and opposite to P, and also a force Q_1 equal and opposite to Q.

The system is in equilibrium; for at the points A, B we have the couple (P, P_1) acting in one direction, and also the couple (Q, Q_1) acting in the other direction; and these couples are equal, for the moment of (P, P_1) is $BD \times Cc$, and the moment of (Q, Q_1) is $AD \times Dd$; and these two products are evidently equal, for they are each equal to the area of the parallelogram A D B C. Hence they balance each other, and the system is in equilibrium.

Now the forces P and Q have a resultant which acts between them; we may therefore replace them by their resultant without disturbing the equilibrium: call it R.

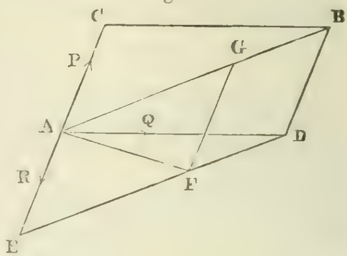
The forces P_1 and Q_1 have also a resultant which acts between them; we may also replace them by their resultant, which we will call R_1.

But these two systems of forces are equal and opposite; and since they balance each other, their resultants must be equal and opposite and also balance each other; and therefore, by axiom 2, the resultants must be in the same straight line.

Hence the resultant of the forces P and Q acting at A must be along the diagonal A B of the parallelogram A C B D whose sides are equivalent to the forces P and Q.

2. *The diagonal* A B *also represents the magnitude of the resultant of the forces* P *and* Q *at* A.

Fig. 2.



For if the diagonal A B does not represent the resultant of P and Q in magnitude, it must either be greater or less than this resultant. Let it be greater, and take A G less than A B to represent the resultant in magnitude. Draw D E parallel to B A. Produce C A to meet D E in E. Draw G F parallel to B D or C E meeting D E in F, and join A F. Then A E F G is a parallelogram, A F is its diagonal, and A E = A C, for both equal B D by construction.

Apply a force R at A along A E equal and opposite to P, and therefore represented in magnitude and direction by A E.

Suppose the three forces P, Q, and R acting at A. We may replace P and Q by their resultant A G. Hence the forces A G and R acting at A must have a resultant acting in the direction of A F.

Therefore P and Q and R at A produce the same effect as a resultant force acting along A F. Now if we remove P and R, which we can do as they are equal and opposite, we have left Q acting along A F as well as along A D, which is absurd.

Hence the resultant cannot be less than A B. In the same way it may be proved that it cannot be greater; and therefore A F must coincide with A D, and the point G with the point B. Therefore the diagonal A B represents the magnitude as well as the direction of the resultant of P and Q.

L. *A Determination of the Specific Heat of Air under constant Volume by means of the Metallic Barometer.* *By* F. Kohlrausch*.

THE value universally assumed for the specific heat of air under constant volume has been calculated from the velocity of sound. There has hitherto been no exact direct determination; for the observations made by Clément and Désormes, as well as by Gay-Lussac and Welter†, can only be regarded as approximations by which the proof has been furnished that

* From Poggendorff's *Annalen*, No. 4, 1869.

† Clément and Désormes, *Journal de Physique*, &c., vol. lxxxix. pp. 321, 428 (1819); Gay-Lussac and Welter in Laplace's *Mécanique Céleste*, vol. v. p. 125. In the first paper all details are wanting which would render possible an opinion as to the accuracy of the experiments. Only *one* experiment is given in full; of all the others only the mean of the results is given.

the magnitude in question is not far removed from that calculated by Laplace. The observers mentioned, as is well known, subjected an enclosed volume of air to a sudden change of density by connecting it for a very short time with a large reservoir of air under a known pressure (mostly that of the atmosphere), and then measured the change in temperature. But as even the most delicate thermometer is too slow to follow rapid alterations of temperature, the enclosed air was itself used as a thermometer by observing the change in pressure which it experienced when the original temperature was restored. As the change in pressure was small, it was measured by a column of water instead of by one of mercury.

Nothing can be urged against the principle of this method. The doubts which might arise from the evaporating water would be removed by the use of sulphuric acid. The question is whether the two assumptions can in practice be simultaneously realized—first, that the duration of the communication with the atmosphere is sufficiently short to justify the neglect of the equalization of temperature which takes place during this time, and, secondly, that after so short a communication the pressure in the receiver is at first exactly equal to the atmospheric pressure. Doubts as to the simultaneous fulfilment of these two conditions will arise at the outset; an empirical proof has not been afforded by the observers. To remove this objection, a knowledge of the condition immediately after the change in pressure must be sought in some other way; and this is afforded by not merely remarking the total change in pressure from the moment of rarefaction or condensation of the mass of air to the final restoration of the original temperature, but commencing the observation very soon after the primary change in pressure. From the course observed, the law of the equalization of temperature will be ascertained by which the condition corresponding to the time zero is to be calculated. The duration of the communication between the receiver and reservoir may be as small as the mechanism of the apparatus permits. Indeed when once this duration is known it can be readily allowed for in the calculation.

Such a method was not applicable so long as the pressure was to be measured by a column of liquid; for in order to diminish capillary actions a tolerably wide tube must be used, and therefore, owing to the initial oscillations of the column of liquid, the most important time for observation is lost.

The metallic barometer now constructed in great perfection fur-

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By a rather arbitrary correction this mean value was brought into accordance with that which followed from the then known velocity of sound. The experiments of Gay-Lussac and Welter, of which Laplace gives an example, do not appear to have been published. Compare also Dulong, Poggendorff's *Annalen*, vol. xvi. p. 454.

nishes a very delicate means of measuring pressure. From the smallness of the mass put in motion in the action of this instrument, the initial oscillations are of very short duration. The moment of inertia of a manometer which is specially intended for such experiments, may be materially diminished as compared with that of the commercial instruments, in which little attention is ordinarily paid to this element of delicacy.

I will here communicate a few observations which I made at the instigation of Professor Weber, which cannot indeed serve for more than a preliminary trial of the method, and should incite to a more accurate repetition with more perfect means.

The instrument used was a Paris barometer graduated in millimetres. It was placed on the plate of an air-pump under a receiver of about 6 litres capacity. The air under the receiver was dried by means of chloride of calcium. By a rapid stroke of the piston, the air in the receiver was rarefied and immediately shut off by a stopcock. The index of the barometer at first moved rapidly towards the smaller numbers, and then retrograded, at first rapidly, and then more slowly through a number of divisions. During this time one observer gave a signal as often as he noted that the index passed over a whole division; a second noticed the corresponding time. When the motion of the index had become slower, parts of a division were noted. After a lapse of sixty seconds a motion could no longer be perceived; that is, the mass of air had assumed the temperature of the surrounding atmosphere.

With the aid of Dr. Nippoldt the six following series of experiments were made, from which a mean may be easily deduced. The diminution in pressure of the air, which before the experiment was under atmospheric pressure, was nearly equal in all experiments; after the original temperature had been restored, the maximum was 38·5 millims. and the minimum 34 millims. As the individual series are proportional, they can all be reduced to the mean alteration in pressure, 37 millims. The observations thus corrected are contained in the following Table, in which t represents the time in seconds which elapses from the beginning of the stroke of the piston, y the distance in millimetres of the index at the time t from its ultimate position.

t.	*y*.	*t*.	*y*.	*t*.	*y*.	*t*.	*y*.	*t*.	*y*.	*t*.	*y*.
seconds.	millims.	seconds.	millims.	seconds.	millims.	seconds.	millims.	seconds.	millims.	seconds.	millims.
2·0	7·1	2·0	8·0	2·0	8·2	2·1	7·4	2·0	7·55	2·3	7·5
4·0	5·2	3·8	5·9	3·6	6·2	3·8	5·5	3·9	5·65	4·1	5·5
5·1	4·2	6·0	3·7	5·1	4·3	6·0	3·6	6·2	3·75	6·1	3·6
8·3	2·2	8·0	2·6	8·1	2 3	8·0	2·6	......	......	8·2	2·6
12·0	1·3	10·3	1·5	11·0	1·4	11·0	1·6	10·1	1·75	10·8	1·6
21·0	0·3	20·0	0·4	19·4	0·4	18·2	0·7	20·2	0·35	18·3	0·6
40	0·1	40	0·2	40	0·05	40	0·1	35	0·15	40	0·1

The calculation of a mean from these individual series is facilitated by the circumstance that the first observation was in each case made about the same time (two seconds) after the commencement of the stroke, and that thenceforward the intervals of time were almost equal. Hence it is sufficient if we take the arithmetical means both of the almost equal times t and of the corresponding values of y.

Thus we find

t.	y.		Difference.
	Observed.	Calculated.	
seconds.	millims.	millims.	millim.
2·07	7·62	7·74	−0·12
3·87	5·66	5·52	+0·14
5·75	3·85	3·88	−0·03
8·12	2·46	2·49	−0·03
10·87	1·52	1·48	+0·04
19·52	0·46	0·29	+0·17
39·2	0·12	0·07	+0·05

The calculated values are obtained thus. Putting the quantity of heat added to the mass of air in each minute proportional to the difference in temperature from the surrounding medium, or, what is the same thing, the alteration in pressure proportional to the difference y of the momentary from the final pressure, we have

$$\frac{dy}{dt} = -\mathrm{A}y, \quad \log \text{nat}\, y = \log \text{nat}\, \mathrm{C} - \mathrm{A}t.$$

We introduce in the calculation for A and C,

$$\mathrm{C} = 11{\cdot}41, \quad \mathrm{A} = 0{\cdot}1877.$$

The calculated values, as we see, agree well with observation. The expression is valid only from the moment at which the stopcock was closed, which was the case at 0·75 second. We get for this time from the formula $y = 9{\cdot}912$.

In order to calculate accurately the amount of heat absorbed from the beginning of the stroke to that time, it would be necessary to have an exact knowledge of the course of the piston; but the correction may be approximately calculated in the following manner:—At the time 0·75 we get the change in pressure due to change in temperature

$$\frac{dy}{dt} = -1{\cdot}860.$$

At the time 0 it was $=0$. Hence as the mean from 0 to 0·75

we may assume

$$\frac{dy}{dt} = -0{\cdot}930\,;$$

from which the change of pressure till then, due to change in temperature, will be

$$-0{\cdot}75 \times 0{\cdot}939 = -0{\cdot}698 \text{ millim.}$$

This number must be added to the value $y=9{\cdot}912$ calculated for 0·75 second, from which the diminution in pressure due to lowering of temperature when the air is rarefied is

$$y_0 = 10{\cdot}610 \text{ millims.}$$

From this we get the ratio of the specific heat under constant pressure c to that under constant density c_1 in the following manner. If the mass of air unity, at the temperature θ, is rarefied from d to d_1 without the access of heat from without, it undergoes a diminution in temperature of

$$\frac{1+\alpha\theta}{\alpha} \quad \frac{d-d_1}{d} \quad \frac{c-c_1}{c_1}\,,$$

if α is the coefficient of expansion of gases with the temperature.

If the residual pressure after rarefaction, but after restoration of the original temperature, be called p_1, the above lowering of temperature produces a diminution of pressure

$$y_0 = p_1 \frac{d-d_1}{d} \frac{c-c_1}{c_1}\,,$$

or, if p is the pressure before rarefaction,

$$y_0 = p_1 \frac{p-p_1}{p} \frac{c-c_1}{c}\,,$$

whence

$$\frac{c}{c_1} = 1 + \frac{y_0}{p-p_1} \frac{p}{p_1}\,.$$

Now in the experiments there was obtained

$$p = 752 \text{ millims.,} \quad p_1 = 715, \quad y_0 = 10{\cdot}61\,;$$

hence

$$\frac{c}{c_1} = 1 + \frac{10{\cdot}61}{37} \cdot \frac{752}{715} = 1{\cdot}302.$$

I have repeated the observations under various conditions —namely with greater and less change of density, with compression of the above mass of air instead of rarefaction, with shorter duration of communication (by rapidly opening and closing the stopcock), finally with three different barometers, one

of which was a small and extremely good English one; and within the limits of accuracy attainable by a single observer I have always obtained the same value. I see no reason why the result should not deserve at any rate the same confidence as the older experiments with the water manometer.

Yet the value found above (=1·302) would be in disaccord with the observed velocity of sound and with the number assumed for the mechanical equivalent of heat; for it would lead to 319·4 metres for the velocity of sound, taking 0·0012934 as the density of dry air at 0° and 756 millims. pressure. Taking, with Regnault, the specific heat of air under constant pressure as 0·2377, the mechanical equivalent of heat would be 532, taking Delaroche and Bérard's number (0·2669) it would be equal to 473 kilogrammetres. The most recent experiments of Regnault have given 330·3 metres for the velocity of sound, from which $\frac{c}{c_1}=1·392$, and the mechanical equivalent of heat (putting $c=0·2377$) would be equal to 437 kilogrammetres. In our experiments y_0 would have to be = 14·5 millims., instead of 10·61, to agree with this result.

It would be difficult to discover a source of error to this amount in the above measurements. It is, however, advisable to repeat the experiments with improved instrumental means. In the latter we should include first of all a method of producing the change of density in a time much shorter, but capable of accurate measurement. Both the motion of the cocks and the observation itself would be best effected by mechanism. Moreover a metallic manometer of as small moment of inertia as possible should be constructed. Doubtless, too, by using a larger receiver with badly conducting sides, the equalization of temperature might be considerably retarded.

I doubt not that if these conditions be fulfilled a trustworthy direct determination may be made by the above method of the ratio $\frac{c}{c_1}$ (and thus an important gap in physics be filled), not merely for atmospheric air, but also (with no greater difficulty) for other gases, which is of especial interest. Apart from this, the indication of this simple method of quantitatively determining with approximate accuracy the heating produced by compression in gas may be welcome to many a lecturer.

Göttingen, January 1869.

LI. *On Fulgurites in the Andesite of the Lesser, Ararat and on the Influence of Local Agents on the Production of Thunderstorms.* By M. ABICH*.

THE influence of the geographical distribution of mountain-masses on the limit-lines between the eastern over-heated (and therefore *over-dried*) steppe-atmosphere of the continent of Asia, and the *moist* and cooler masses of air brought by north-west atmospherical currents, is nowhere so conspicuous as within the region of the Great and Lesser Ararat group, where it finds its highest expression in the beginning of the æstival half of the year, under the form of frequent and sudden thunderstorms in the summit-region. These phenomena stand in close relation with the orographical constitution of the mountain-group. The first clouds and the first electrical discharges within them begin generally on the north-west side of the group, where its most powerful massif reaches furthest into the region of the Araxes valley, conspicuously spreading in breadth. The thunderstorm, in its rapid development, soon envelopes in a south-east direction the whole top region of the mountain, remaining stationary within the space between the Great and the Lesser Ararat, the north-west high portion, called "Kippgöll," standing at the same time in full sunlight. After a shorter or a longer space of time the thunderstorm dies away on the Lesser Ararat, or it descends with gradually diminishing energy into the plain towards Nachitshevan and Dzaulze. These well-characterized and regular thunderstorms begin in April (old style), reach their maximum in May, and have considerably diminished in the course of June. Although rare in July and August, they may possibly break out suddenly during this period, and be thus an obstacle to ascending Ararat. The journal of a meteorological station at Erivan, established by M. Abich and continued during more than fourteen months, registers for April 10, for May 14, and for June 6 several thunderstorms in this Ararat region, not mentioning those which had broken out in the intervals of the hours (six every day) fixed for the observation of the instruments.

M. Abich, having repeatedly ascended the Lesser Ararat, has been enabled to ascertain some physico-lithological facts demonstrating the frequence of thunderstorms in these lofty regions, and of the mutual action of atmospherical and terrestrial electricity. The chief rock of the Lesser Ararat is a fine-grained amphibolic andesite, rising in cliffs above the slopes covered with decom-

* From a letter to Chevalier W. de Haidinger, dated Tiflis, June 25, 1869. Communicated and translated by Count Marschall, F.C.G.S. &c.

posed andesite, or in obtuse pyramidal massifs, on the margin of a fault across the mountain, thus constituting its extreme top, 12,106 feet above the sea-level, according to the measurements taken by M. Abich in $18\frac{44}{45}$. When ascending the mountain from its easier, north-west side, M. Abich saw on the upper slope some dark stripes on the light-brown rock, whose vitrified aspect was evidently due to the action of lightning. The path of the electrical discharge was constantly traced in the form of a narrow tube, in the form of a thick goose-quill, traversing the rock, and lined on its inside with a dark green vitreous slag. These tubes increase in number towards the top, and have modified a portion of the top itself into a variety of andesite, which may properly be called "fulguritic." The originally compact rock of microcrystalline texture, traversed in every direction by vermiform fulgurites bearing evident marks of igneous fusion, has taken a cavernous aspect not unlike wood completely disaggregated by the borings of *Teredines*. The depth to which the rock had been attacked by lightning could not be sufficiently ascertained. M. Abich's laborious examinations of the top of the Great Ararat could not discover there any traces of fulgurites, either on the cliffs of black trachytic porphyry on the steep south-east slope of the upper cone, reaching an absolute altitude of 13,000 to 14,000 Paris feet, or on the reddish-brown scoriaceous rocks rising above the snow on the margins of the flattened top. An investigation of the north-west side of the Ararat, between the Kipp-Göll and Professor Parrot's encampment, 12,954 Paris feet above the sea-level, led to the same negative result. The investigation of the upper region of the south slope proved more satisfactory. The first fulgurites were observed on the massive trachyte cliffs at the mouth of a deep-cut glacier-ravine, the only real valley on the south side of the Ararat, exactly coinciding in longitudinal direction with the Valley of St. Jacob on the north-west side. The slight depression of the top line of Ararat, as its projection appears when seen from the north, would coincide with the defile between these two valleys running in opposite directions. The absolute altitude of the glacier's termination in the first-mentioned ravine is 11,200 feet according to M. Abich's statements, based on corresponding barometrical observations made at Erivan and Nachitshevan.

Another trace of fulgurites has been noticed in the Goëll-Dag, as the Jessidian Kurds call a conspicuous conical eminence visible from Bajazid, on the same apparent level as the south-west side of the Ararat. This eminence is about $1\frac{1}{2}$ hour's march distant from the flatly vaulted plateau of the Kipp-Göll (10,648 Paris feet above the sea-level). The Goëll-Dag is the highest point of a rocky ridge diverging from the main mass of Ararat nearly

on the horizon of permanent snow, and stretching downward in a N. 35° E. direction. Its component rock is a light-coloured phonolite-like, fine-grained trachyte, separating into sonorous laminæ, quite different from the dark-coloured doleritic lava covering the mountain-slopes. A similar ridge, at some distance from the first, and somewhat diverging from it, runs from the top ridge of the Ararat down to the lower region. These ridges are undoubtedly the upheaved margins of the powerful fissures traversing the foundations of the Ararat mass, probably coeval with its last great upheaval, and antecedent to the great effusion of lava attending it. The whole structure of the Ararat slope confirms this view. From the Goëlldag (11,340 Paris feet above the sea-level) the eye looks down into the broad, valley-like space between the two rocky ridges, which converge upwards and at a short distance towards a third ridge. In this place the dolerite is covered by glacier-detritus; and a large current of lava, descending in a south-west direction, having advanced in the form of a wall on the plain of Bajazid, had evidently found here a fissure or excavated bed. Another current of lava, reaching the plain in the direction towards Bajazid, seems to have also broken out alongside of this second rocky ridge. The only traces left by lightning in these regions are isolated traces of fusion and perforations of trachyte plates. No such traces had been ascertained on the north side of the Ararat.

Isolated fulgurites occur on the Parlydag ("Mountain of Lightning" in the Tartar language), an extensive trachyto-porphyritic system, dominating the plateau of Sinak, on the nitrachytic top of the Magaz*, and on the highest top of the Sahand near Tawris (Adherbeidjan) at an altitude of 11,600 Paris feet. The light-coloured vitreous and lithoid rhyolites, forming the prominent tops of the Agdag and Boosdag mountain-systems (11,168 and 10,726 Paris feet above the sea-level), offered no traces of fulgurites; nor did the crater-margin of the great eruptive trachytic system of the Ischichlydag (9740 feet), or the Tardoureck, a flatly vaulted cone south-west of the Ararat behind Bajazid.

All these details are necessary for demonstrating the frequence of thunderstorms in the region of the Lesser Ararat, and the very frequent and intense action of lightning perceptible on its summit, to be facts depending not only on general physico-geographical circumstances, but still more on the situation of this mountain-system relative to the plain of the Araxes and to the Great Ararat.

* Altitudes measured by M. Abich:—plateau of the Sinak, 7382 Paris feet; uppermost peak of Parlydag, 6887 feet. Uppermost peak of the Magaz (Imperial Russian Staff-Corps), 12,610 Paris feet.

If we suppose the Pontic atmosphere, coming from W.N.W. at considerable altitudes, to pass over the Taurian Highlands, radiating heat in consequence of protracted insolation, it must become saturated nearly to its maximum with aqueous vapour and receive a notable amount of negative electricity. Whenever this atmosphere meets with the colossal prominence of Ararat, the electricity of the clouds, accumulated in the aqueous vapour, is suddenly increased; and, of course, electrical compensation begins first on the north-west side of the Great Ararat. The elliptical form and the situation of summit and ridge of Ararat force the accumulated atmospheric current coming towards its side to the broad elevated valley between the two Ararats, and to its opening into the region where the atmosphere arising from the hot south portion of the Araxes-plain has reached its maximum of heat and dryness.

The greater half of the Lesser Ararat, whose base is notably inclined east-north-eastward, and which rises to more than 9000 feet above the plain of the Araxes*, is almost to its summit under the action of this *pure and non-electric* atmosphere, moving constantly south-eastwards, and counteracted by a cold north-west current descending from the depression between the two mountain-groups (altitude 8274 feet). The notable energy of this counter-current is a necessary consequence of the local thermal contrast between the summit-region and the neighbouring heated plain. The uncommonly rapid decline of temperature observed on the higher horizons of this valley is indicative of an accelerated fall of the higher strata of clouds, containing (as M. Vogel supposes) aqueous vapour of a temperature far below the point of congelation, and the presence of which causes the violent falls of *hail*, attending in most cases the thunderstorms breaking out in the lower half of the valley. The clouds, highly charged with electricity, coming rapidly from the Great Ararat and turning round the mountain, discharge each other on the north and east side of the Lesser Ararat, as the difference between the temperature and the point of degelation of the air in those regions increases with the distance from the mountain towards the plain. At the same time the increased permanent electro-negative tension of the summit of the Lesser Ararat discharges the latent electricity of the vapours, and provokes a continued intense compensation with the electrically charged clouds constantly coming from the Great Ararat. At all events, the degree of freedom from vapour of the atmosphere above the Nachitshevan half of the Araxes plain, as resulting from preceding meteorological conditions, and consequently its degree of electrical conductibility, must

* Absolute altitude of the Araxes plain in the meridian of the Great Ararat, about 2400 feet.

cause the thunderstorm rising in the Ararat region either to exhaust itself in the Lesser Ararat, or to pass it rapidly and to spread over the whole opposite plain. The facts and observations above mentioned seem to confirm MM. Peltier and Lamont's views on the origin of thunderstorms and of atmospheric electricity.

LII. *Hailstorms in Russian Georgia.* *By* M. Abich*.

[With a Plate.]

THE first of these storms took place May 27, 1869, at 3 P.M., the other June 6, at 6 P.M., both within a limited region of the Trialat Mountains near Beloi Kliutsch, about forty wersts (26½ Engl. miles) from Tiflis. The hailstones, although different in form in both cases, were of uncommon size, and deserve some attention. In the first case they presented a quite regular flattened spheroidal form, somewhat like the so-called "mandarin-oranges," and a series of varieties almost reminding one of organic evolution. The second case was a complete "shower of ice crystals"—not of fragments of ice of indistinctly crystalline outlines, but of spheroidal crystalloid solids, densely but irregularly beset, on the surfaces corresponding to their longitudinal diameter, with limpid regular crystals showing various combinations of forms belonging to the tri- and monoaxial systems—a peculiarity which, it seems, has not yet been observed, or at least published. The forms characteristic of calcareous spar and of specular oxide of iron prevailed, especially the scalenohedron, combined with rhombic planes, in crystals 15 to 20 millims. in length. Other crystals exhibit the prism, combined with obtuse rhombohedra, and with the terminal plane perpendicular to the principal axis. Some specimens that fell soon after the beginning of the storm were aggregations of tabular crystals, 30 to 40 millims. in diameter, resembling the rosette-like aggregations of specular oxide of iron from Mount S G otthardt.

Both these storms caused enormous devastations; strong branches were struck down as if cut with some sharp implement. The specimens gathered immediately after the fall presented perfectly sharp edges and somewhat convex surfaces, like some crystals of diamond—except the scalenohedral surfaces, which were completely flat. M. Abich made drawings from ten of the most remarkable and best preserved specimens, intending to publish *in extenso* his observations on the phenomena in question. These hailstorms have a close connexion with the abnormal me-

* From a letter to Chevalier W. de Haidinger, dated Tiflis, June 25, 1869. Communicated and translated by Count Marschall, F.C.G.S. &c.

teorological conditions observed in Georgia during June 1869, and characterized by uncommonly intense frequent rains and thunderstorms. On June 20, a hailstorm, still more violent than those of May 27 and June 6, caused horrible devastations in the valley of Manglis, 18 wersts from Tiflis, and progressed, in the form of rain and electrical discharges, as far as into the valley of Algat.

Pl. III. figs. 9 and 10 are intended to represent the outlines of two of the most remarkable varieties of hailstones as true to nature as possible, without any pretence to elegant execution. In the two cases under notice, personal observation sets more or less at defiance any theory of the formation of hail hitherto established. How could indeed the formation of such crystalline aggregations, as regular as those of the calcareous spars of Andreasberg, be possible in the midst of the tumult generally supposed to be necessarily connected with the formation of hail? These aggregations may have had a long stay within a medium of highly refrigerated aqueous vapour before they fell to the ground. It must be remarked, to fully understand the drawings, that the shaded portion of the flattened spheroidal fundamental form of the groups is not always opaque in the original. Only the circle round the centre has a milky aspect, due to the air-bubbles enclosed in it, as also the nucleus of the greater number; in other specimens the nucleus is transparent, especially when reduced by melting away into disks of $\frac{3}{4}$ to 1 inch in diameter, sometimes affecting the form of a perfect regular hexagon. In this case the milky circle around the centre appeared distinctly as an intricate tissue of minute lengthened pores and of capillary fissures filled with air. The shadow next to the margin of the larger peripherical circle is only intended to indicate the rounded and flattened spheroidal form of the chief body, on whose broader margin the crystals themselves adhere parasitically, or are inserted, as in an alveole, made visible by the commencement of fusion (see *a* in fig. 9). All the specimens presented lengthened vermiform and pyriform pores filled with air, extending radially from the centre to the circumference. The drawing shows these pores of approximately natural size.

LIII. *On Electrification.*
By Thomas T. P. Bruce Warren*.

WHEN an insulated wire or cable is connected to a battery, and the deflection noted on a galvanometer, the first rush of current into the cable is due to the electrostatic capacity

* Communicated by the Author, having been read at the Exeter Meeting of the British Asaociation, in Section A, August 1869.

of the insulator. Battery-contact being still maintained, the deflection falls very rapidly at first, and gradually becomes reduced for some time after.

The shorter the length of cable and the lower the degree of insulation, the less defined will be the differences in the deflections after a few minutes' contact.

Great care must be taken, when making these experiments, that the cable has not been previously charged; should the cable have been charged, it must be connected to earth for some hours before testing. The battery must be in very good condition, and unsteady deflections totally discarded.

The ratio between the deflections for equal periods of contact is independent of the length, and is greater or less according to the specific resistance of the dielectric.

The ratio is unaltered under different electromotive forces so long as constancy is maintained during the time of observation and the deflection itself the same with the different electromotors at the end of the first period of contact; but when, with different electromotive forces, the deflections at the end of the first period of contact are not the same, we may obtain the deflections which should be given on prolonged contact if we know the deflection for a corresponding period by any electromotive force, since the deflections for the first period of contact will have to one another the same ratio which the deflections at any other period of contact have: thus if with a given electromotive force we obtain at the end of the first minute's contact a deflection of 84, which at the end of the second minute is reduced to 76, and with a different electromotive force we have a deflection of 70 at the end of the first minute's contact, the deflection at the end of the second minute will have the same ratio to 76 which 70 has to 84.

Under different temperatures the resistances corresponding to one, two, three, &c. minutes' contact follow the same law of variation. Thus if $R = r \times \text{constant}_t$ represent the resistance after one minute's contact, then

$R' = r'$	$\times \text{constant}_t =$	resistance	after	2nd	minute.
$R'' = r''$	"	"	"	3rd	"
$R''' = r'''$	"	"	"	4th	"
$R'''' = r''''$	"	"	"	5th	"
$R^n = r^n$	"	"	"	nth	"

$r, r', r'', r''', r'''', r^n$ are the resistances determined after 1, 2, 3, 4, 5, n minutes' contact respectively, and $R, R', R'', R''', R'''', R^n$ the required resistances for the same differences of temperature t, and at the end of 1, 2, 3, 4, 5, n minutes' contact.

If at any temperature T we obtain a deflection G after one minute's contact, which at the end of the second minute falls to

g, we may calculate what the deflection should be at the end of the second minute for any other temperature by knowing only the deflection after the first minute at this temperature.

Let G and g be the deflections after one and two minutes' contact respectively at a given temperature, and G$'$ the deflection at the end of the first minute at any other temperature, then $G : G' :: g : g'$; g' will be the deflection at the end of the second minute at this temperature.

By calculating in this way the value of g', and comparing it with the actual reading, much more reliance can be placed on the value of a test than can be done by correcting for temperature in the usual way. We are thus quite independent of temperature for knowing whether a cable or core has received the slightest injury in manufacture.

G and g may readily be obtained by testing a core at a fixed temperature, as 75° F., which is now done.

Coils having the same dimensions have rarely the same ratio in their resistances on prolonged contact with a battery; but when several coils are joined together, the ratio between the deflections for any two successive durations of contact may be obtained from the reciprocals of the deflections of the several coils.

In reducing tests of insulation by discharge to measures of resistance, it is impossible to obtain but approximations in the ordinary way of making the tests. The best way is to charge the cable or core for one minute and then note the discharge, recharge the core, and take the instantaneous discharge. By this method we know exactly the amount of electrification which has been given to a core; but by taking the instantaneous discharge first, even although contact with the battery is made for one minute, we cannot say how much electrification is retained in the core.

When a core is thus connected to a battery for one minute and afterwards removed, electrification still takes place, but, of course, not precisely as if connected to a battery; for the insulator, instead of being acted upon by a constant charge, is affected by the variable charge consequent upon leakage; but when the core is held free for one minute, it is very easy to ascertain how much effect the electrification has had in reducing the loss.

The amount of electrification retained at any given interval is proportional to the quantity of charge remaining at that time. The longer battery-contact is maintained, the slower will a core or cable lose its charge, and conversely.

In a cable which has been charged by contact with a battery for one minute and afterwards held free for one minute, the electrification will be the same as if, instead of being held free, it had been left connected to a battery having the last tension, thus:—

If the discharge after one minute's contact and one minute's insulation be 180, and the immediate discharge 200, the duration of contact being also one minute, the total effect for electrification at the end of the minute's insulation will be 95 per cent. of what it would have been if connected to the same battery for two minutes.

By taking these considerations into account, the formula of Professor Fleeming Jenkin, $R=\left(\frac{t}{K\log_\epsilon\frac{C}{c}}\right)\times 10^6$, may be rendered strictly applicable for deducing from the loss of static charge in time t the resistance for the same period of contact in absolute measure, or in terms of that system which makes R and K functions of each other; and we may expect that the capacity K can be eliminated from this formula when R is known, if we can determine the constant for electrification for the interval of time during which the core is held free.

In this formula, if the test is performed in the manner here indicated, t will be 60, and the value obtained for R will be the resistance at the end of the second minute more nearly as $\frac{C}{c}$ approaches 1. This resistance has then to be divided by a number which expresses the ratio between the first and second minute's contact; approximately, and on short lengths of core, this may be obtained as follows:—Recharge the core, after being kept to earth for some hours, maintaining contact with the battery for two minutes before noting the loss; then by dividing the percentage of loss in the first experiment by the percentage of loss given in the second experiment, we shall obtain a number by which, if R be divided, the resistance corresponding to one minute's contact may be found.

The following ratio expresses the rate of increase in resistance on prolonged contact:—Let D be the deflection at the end of the first period of contact, and d the deflection at the end of the nth period, then D : d : : d : deflection at the end of n^2 minutes; or the deflection after the first period of contact is to the deflection for any other period of contact as this deflection is to the deflection at the period of contact corresponding to the square of the intervals.

I have to acknowledge my obligation to Mr. Hooper for placing at my disposal the necessary instruments and cores for the subject of this paper.

LIV. *Experimental and Theoretical Researches into the Figures of Equilibrium of a Liquid Mass without Weight.*—Eighth Series. *By* Professor J. PLATEAU*.

Researches into the causes upon which the easy development and the persistence of liquid films depend.—On the superficial tension of Liquids.—On a new principle relating to the surfaces of liquids.

IN the last series of these researches, while discussing the various processes of producing liquid films, I tried to make it clearly understood that the production of such films always depends upon the cohesion and viscosity of the liquid—the former property opposing the rupture, and the second impeding the relative motion of the molecules when the liquid has reached a certain degree of thinness, and thus rendering any further attenuation of it more slow. I concluded, in consequence, that the property of undergoing extension into thin films must belong to all liquids, and I tried to show that this is really the case.

But if all liquids are capable of being spread out into thin films, they nevertheless present important differences in the degree of facility with which the films are formed, and in their permanence when produced. For example, it is easy to blow large bubbles at the end of a pipe with soap and water, but no one would think of trying to do so with pure water. The easy extensibility of solution of soap and of some other liquids into thin films of great size is generally ascribed to their viscosity; but I find that viscosity, at least as commonly understood, plays only a quite subordinate part in this facility of extension. In fact experiments, which will be spoken of further on, show that the viscosity of a solution of 1 part of Marseilles soap in 40 parts of water, a solution with which bubbles can be blown more than 25 centims. in diameter at the mouth of a common tobacco-pipe, is scarcely greater than that of pure water; moreover one part of the same soap in 500 parts of water is sufficient to give bubbles a centimetre in diameter; and, lastly, the fat-oils, glycerine, whether pure or mixed with water, treacle under the same conditions, and solutions of gum-arabic of various degrees of concentration, liquids which are all of them more viscous than solution of soap, are absolutely incapable of being blown into bubbles at the mouth of a pipe. We must consequently look elsewhere for the cause of the phenomenon: this is what I do in the pre-

* Translated from the *Annales de Chimie et de Physique*, S. 4. vol. xvii. p. 260. For abstracts of the previous series see Taylor's Scientific Memoirs, vol. iv. p. 16, vol. v. p. 584; and Phil. Mag. (S. 4.) vol. xiv. p. 1, vol. xvi. p. 23, vol. xxii. p. 286, vol. xxiv. p. 128, and vol. xxxiii. p. 39.

sent series; and it will be seen that the cause in question seems to reside in the most mysterious properties of liquids.

I begin by the study of an element the influence of which must be regarded as self-evident—namely, the tension of liquid surfaces, a curious property whose existence has long remained a mere hypothesis. In order to place this matter in a clear light, I first of all give an historical sketch of this hypothesis, passing in review the researches of Segner, Leidenfrost, Young, Hough, MM. Henry, Hagen, Lamarle, Dupré, Van der Mensbrugghe, and Quincke; I also recall my general principle in relation to systems of films, and from the whole I draw the following conclusions:—

1st, tension really exists in every liquid surface, and consequently in every liquid film; 2nd, this tension is independent of the curvature of the surface or of the film; it is the same throughout the whole extent of the same surface, or of the same film, and at each point it is the same in all tangential directions; 3rd, it is independent of the thickness of the film, at least so long as this thickness is not less than twice the radius of the molecular attraction; 4th, it varies with the nature of the liquid; 5th, in the same liquid it varies in the opposite direction to the temperature, but at ordinary temperatures it undergoes only small alterations; 6th, we possess a great number of processes for measuring this tension.

The tension continually tends to break the films; but, according to the third conclusion above, this tendency is no stronger in a very thin film than in one that is comparatively thick. Consequently, if very thin films break in reality more easily than thicker ones, it is no doubt because they offer less resistance to external causes of rupture, such as movements of the air, slight shakings, &c.

In the case of most liquids, films that are at all large burst as soon as they are formed. In order to be able to make observations on films of a great number of liquids, I have therefore been obliged to confine myself to films of small size; and I have chosen for the purpose of examination the hemispherical bubbles formed at the surface of liquids by the ascent of air, studying those only the diameter of whose base was between 10 and 12 millims. When the liquids under examination were more or less volatile, like water, aqueous solutions, alcohol, &c., the observations were made in an atmosphere saturated with its vapour; and when, on the contrary, they had a tendency to absorb moisture, like glycerine, sulphuric acid, &c., they were made in a dried atmosphere.

These experiments have led me to divide liquids, in relation to their formation of films, into three principal categories. The

general characters of the first are the formation of little or no froth when shaken, the incapability of being blown into bubbles, the absence of colours on the hemispheric bubbles, or a tardy and only incipient coloration, showing only the red and green of the last orders. Among the numerous liquids which belong to this category, I may mention water, glycerine, sulphuric and nitric acids, ammonia, saturated solutions of tartaric acid, nitrate of potassium, carbonate of sodium, and chloride of calcium.

The liquids of the second category are distinguished from the preceding by the prompt and decided coloration of their films, showing tints of all the orders. These liquids are the fat oils, lactic acid, glacial acetic acid, oil of turpentine, alcohol, benzine, Dutch liquid, chloroform, sulphuric ether, sulphide of carbon*, and no doubt many more.

The liquids which belong to the third category are covered over, when shaken, with an abundant and very persistent froth; they can be easily blown into bubbles at the end of a pipe; the hemispherical bubbles which they form last much longer than those formed by the liquids of the two preceding categories, usually for several hours, and sometimes even for several days. They have generally at first a well-marked colourless phase, the duration of which differs much in different liquids; they then become gradually coloured, but in a way which varies somewhat with the nature of the liquid.

This category is not numerous: if we take away some substances which are only liquid when hot, such as glass, it is reduced essentially, I think, to the solutions of different kinds of soap, of saponine, and albumen, to which may be added solution of sesquiacetate of iron.

In order not to make this abstract too long, I omit a series of curious facts that have been met with in the course of the experiments, and an account of which will be found in the memoir. I pass on to the deductions which have an immediate bearing upon the question I am discussing.

We have seen that films of the second category assume, immediately on their formation or very soon afterwards, bright colours belonging to all the orders; whence we must conclude that they get thinner with extreme rapidity.

We have seen also that there is never an immediate or nearly immediate coloration in the films of the first category: the very great majority remain colourless till they break; in the very rare cases in which such films do become coloured, this does

* At ordinary temperatures, the hemispherical bubbles of sulphide of carbon, which last only a fraction of a second, do not exhibit colours; but at a few degrees below zero a bright coloration may be observed on some of them.

not happen till after several seconds, sometimes not till after two minutes. It evidently follows from this that in this category, on the contrary, the diminution of thickness is very slow.

Again, we have seen that the films of the third category have generally a long colourless phase, and that the coloration that appears afterwards never changes quickly. It follows from this that in the third category, as in the first, the diminution of thickness takes place very slowly.

This great difference in the rapidity with which films of the second category diminish in thickness as compared with those of the other two, cannot be attributed to ordinary viscosity; for the fat oils and lactic acid, for instance, which belong to the second category, are much more viscous than most of the liquids belonging to the first and second; oil of turpentine, again, which belongs to the second category, is more viscous than water, which belongs to the first, &c. Now the distinguishing character of a film is the great extent of its surfaces in proportion to its volume; we are consequently forced to recognize here an effect depending on the faces of the film, and to look for the cause of the great difference in question in a viscosity peculiar to the superficial layers, and independent, or nearly so, of the internal viscosity, and which is very weak in the liquids of the second category, but, on the contrary, is very strong in those of the first and third.

This principle being admitted, let us apply it to the phenomena. Take a hemispherical bubble at the moment of its formation, and let us fix our attention upon one of the two faces of the film, on the convex face, for example, and let us imagine it divided into horizontal molecular rings from the summit to the base. All these rings descend, and consequently each of them goes on always increasing in diameter; this implies that its molecules separate further from each other, and that other molecules belonging to the subjacent layer come and place themselves in the intervals, so as to reestablish a uniform arrangement. This must evidently apply also to the concave face. Let us now consider one of these molecular rings at the moment of its departure from the summit; it is clear that for any small space traversed there is a great increase of the distances between the molecules of this ring; and it will be easily admitted besides that these movements are not performed with mathematical regularity, and hence that in the same ring the intervals between the molecules are not all absolutely equal. This being admitted, let us suppose that from some cause or other an obstacle interferes with the free arrival of the subjacent molecules into the intervals; one or other of these will in this case soon become so great that the attraction of the molecules which it separates

is no longer able to counterbalance the tension; these molecules will then easily drag after them their inside neighbours, which will thus be separated in their turn also; the separation will gradually get deeper and deeper, and the film will break at this point. Now in hemispherical bubbles of the first category the superficial layers have, according to my principle, a very great viscosity, so that molecular movements take place with difficulty; hence it is intelligible that very near to the summits of either of the faces an increased molecular interval may not have time to be filled up before the tension, if at all energetic, causes rupture as above. Such is, in my opinion, the explanation of the breaking of nearly all the bubbles of the first category before any coloration is visible upon them.

It will now be seen why it is impossible to blow bubbles with films of this category—namely, because the film cannot extend in consequence of the blowing, unless the molecules of its two faces get continually further apart, thus making room in the intervals between them for molecules nearer the inside of the film, and giving numerous opportunities for the film to break.

In the films of the second category the rupture must be incomparably more rare. In this case, according to my principle, the molecular mobility of the superficial layers is very great, and consequently there is little hindrance to the movement of the interior molecules into the widened intervals between those at the outside; hence films of this category become in a very short time extremely thin. This rapid attenuation teaches us why we cannot succeed in blowing bubbles with these liquids any more than with those of the preceding category. When we have taken up a plane film at the end of the pipe, the suction due to the small quantity of liquid which adheres to the circumference of the pipe-bowl, and the descent of the liquid due to the mouth of the pipe not being held perfectly horizontal, make a film of this kind almost instantaneously so thin that it often bursts by the unavoidable movements of the hand before it is possible to put the pipe to one's mouth; and when this does not happen, the bulging of the film produced by blowing and the descent of the liquid towards the lowest point soon bring about the same result.

We now come to the third and most important category, that of the liquids which admit of being blown into bubbles. Here, as in the first category, the superficial layers have but little molecular mobility, so that such films become thinner only slowly; but they seldom break, because, notwithstanding the descent of the liquid and the effect of the blowing, the films subsist and are capable of undergoing great extension. If the ideas above explained be admitted, we must conclude that in liquids of the

present category the tension is insufficient to cause rupture; and this is supported by a comparison of the respective tensions of water and of our solution of Marseilles soap: the tension of a film of water at the common temperature is 14·6, and that of a film formed by a solution containing one part of Marseilles soap to forty of water is only 5·61*, or between one-half and one-third of the former.

Nevertheless, in order that a liquid may be capable of extension into bubbles, it is not indispensable that the tension should be absolutely weak, if only it is so in comparison with the viscosity of the superficial layers, or, in other words, if the ratio of the superficial viscosity to the tension be sufficiently great. For instance, while the tension of a film of soap-water, as we have just seen, is only 5·61, that of a film of a solution of albumen, made by adding a tenth of its volume of water to white of egg, is 11·12, or twice as great; but in hemispherical bubbles of soap the colourless phase is at most twenty seconds, while in those of albumen it lasts several hours. Thus when we pass from the first of these liquids to the second, the tension, or the force tending to break the films, becomes double; but the resistance to rupture increases at the same time, in consequence of the greater viscosity of the superficial layers, and thus solution of albumen stretches out into bubbles like soap, but to a less degree.

Such is the theory which I propose as a solution of the principal question treated of in the present series of these researches. In order that a liquid may be capable of forming large and persistent films, and may consequently admit of being blown into bubbles, it is necessary, in the first place, that the viscosity proper to the superficial layers of its films should be great, in order that the diminution of thickness may take place slowly; it is also needful that the tension should be relatively small, in order that it may not overpower the resistance opposed by the above viscosity to the rupture of the film, when, in consequence of superficial movements, a more than ordinary separation of the molecules occurs. I have shown, however, by reasoning which is too long to be dwelt upon here, that the ratio between superficial viscosity and tension, which makes the formation of bubbles possible, must be greater in proportion as the superficial viscosity is greater.

I next pass to a series of facts in support of this theory. I have tried, in the first place, to prove by direct experiments the existence of a viscosity peculiar to the superficial layers, and the variations which it presents in different liquids. The following is, in substance, the method of experimenting that I adopted, and which I found perfectly successful.

* These tensions are expressed in milligrammes per millimetre of length.

A pivot, 25 millims. high, carrying a magnetized needle 10 centims. long, was fixed at the centre of a cylindrical glass dish, 11 centims. in internal diameter and 6 centims. deep. In making an experiment, the liquid to be examined was poured into the dish until it just came up to the lower face of the needle; next, by means of a bar-magnet, the needle was turned through 90° from the magnetic meridian, and kept in that position until the surface of the liquid had again become motionless; then the bar-magnet was suddenly removed and the time observed that the needle took in traversing a given angle: in my experiments this angle was 85°. When this time had been observed, more liquid of the same kind was added until the needle was covered to a depth of about 2 centims., the interior of the cap of the needle was freed from the small quantity of air which it contained, and under these new conditions the time occupied by the needle in traversing the angle of 85° was determined as before.

Experiments of this kind were made with five liquids of the first category, namely, water, glycerine, and saturated solutions of carbonate of sodium, nitrate of potassium, and chloride of calcium. Now, although it would seem that the needle must experience about twice as little resistance at the surface of the liquid as it does in the interior, nevertheless for each of the above liquids its velocity was much less in the former case than it was in the second. With water, for instance, in one series of observations the mean time occupied in traversing 85° at the surface was 4·59 seconds, while in the interior it was only 2·37 seconds. Consequently it is evidently necessary to assume that the surface of these liquids opposes a special resistance to the movement of the needle, or, in other words, that the superficial layer possesses a viscosity proper to itself and much greater than the interior viscosity. We may add that if, while the needle is kept at the surface at an angle of 90° from the magnetic meridian, any very small light body, such as the smallest fragment of gold leaf, is laid on the surface of the liquid in the meridian, on setting the needle free, this small body is seen to be displaced and to move in the same direction as the needle, whence it follows that the whole surface of the liquid turns together with the needle.

Five liquids of the second category, namely, alcohol, oil of turpentine, olive-oil, sulphuric ether, and sulphide of carbon, were tried in the same way; and for each of these the velocity was, on the contrary, greater at the surface than in the interior. With alcohol, for example, the average time occupied by the needle in traversing 85° was 1·48 second at the surface, and 3·30 in the interior. Moreover, in the case of these liquids, a small body floating on the surface in the magnetic meridian

was in no way disturbed by the movement of the needle, which simply came and struck against it. It follows from this that in liquids of the second category the superficial layer has not any greater viscosity than the interior; but I have shown that in reality it has less. I will confine myself here to citing a single fact bearing on this point. If the experiment of a small floating body is made with a mixture of equal volumes of water and alcohol, the body is simply struck by the needle; thus the excess of superficial viscosity possessed by the water is completely destroyed by the presence of the alcohol. It therefore follows that the superficial layer of the latter must be less viscous than the interior, or, if I may so express myself, that it possesses a negative excess of viscosity which neutralizes the positive excess belonging to the water.

Lastly, five liquids of the third category were tried, namely, solutions of Marseilles soap, soft household soap, resin soap, saponine, and albumen, and showed, like those of the first category, a superficial viscosity much greater than the interior viscosity. One of them (solution of saponine) yielded in this respect extraordinary results; its superficial viscosity is extremely strong: the needle placed at 90° from the magnetic meridian and then left free remains in this position, as if the liquid were covered with a solid pellicle; but yet it is impossible to detect by any means the presence of such a pellicle. Solution of albumen shows a similar behaviour, but in a less degree.

Thus the results obtained by means of the magnetic needle in regard to the fifteen liquids that I have submitted to this kind of trial, fully confirm the consequences drawn from the experiments on the hemispherical films; we may therefore, I think, look upon the following principle as fully established:—

The superficial layer of liquids has a proper viscosity, independent of the viscosity of the interior of the mass. In some liquids this superficial viscosity is greater than the internal viscosity, and often much greater, as in water and, especially, in solution of saponine; in other liquids, on the contrary, it is less than the internal viscosity, and often much less, as in oil of turpentine, alcohol, &c.

The idea of a viscosity proper to the superficial layer of liquids had already been put forward by M. Hagen; but he seems to consider that this viscosity is greater in all liquids than the internal viscosity.

In order to be able to form a definite estimate of the relations between superficial viscosity and tension, we should require to have some accurate means of determining the numerical values of the first of these elements, in the same way as those of the second are determined. I have tried without success to find an accurate method for this purpose; but I have shown that, in the

case of those liquids of the first and third categories in which the superficial viscosity does not greatly exceed that of water, we may adopt as approximate relative values the ratios between the times occupied by the movement of the magnetic needle at the surface and in the interior; a small correction, however, must be applied to this ratio in the case of liquids like glycerine, in which the internal viscosity is very great. I have therefore calculated these ratios; then representing the superficial viscosity of water by 100, I have expressed those of the other liquids in the same units; and, lastly, I have divided the numbers so obtained by the respective tensions of the films, and have thus formed the two Tables which follow:—

First Category.			
Liquids.	Superficial viscosity.	Tension of films.	Ratio of superficial viscosity to tension.
Water	100·00	14·60	6·85
Price's glycerine	60·42	8·00	7·55
Carbonate of sodium (saturated solution)	91·14	8·56	10·65
Nitrate of potassium (saturated solution)	96·35	11·22	8·59
Chloride of calcium (saturated solution)	90·62	11·06	8·19
Third Category.			
Solution of Marseilles soap, 1 : 40	94·79	5·64	16·81
,, soft household soap, 1 : 30	96·95	6·44	14·96
,, potash resin-soap	84·89	7·68	11·05
,, saponine 1 : 100	Not determined, but extremely great.	8·74	Not determined, but extremely great.
,, albumen	Idem.	11·42	Idem.

It will be seen, on looking at these Tables, that the ratios of superficial viscosity to tension are all greater for the liquids of the third category (that is to say, for those which yield bubbles and a copious froth) than for those of the first category, and moreover that, with a single exception, the difference is considerable.

In the second place, of the liquids in the first Table, that one for which the ratio of these two elements has the highest value (namely solution of carbonate of sodium) is precisely the one which, when shaken in a flask, yields the most perceptible froth; we may therefore suppose that if a saturated solution of carbonate of sodium is incapable of forming bubbles, it is not so far from having that property as the four other liquids.

In the third place, among the liquids of the second Table, the one which shows the smallest ratio is solution of resin-soap, and this is also the liquid in which bubbles attain the smallest size.

The small difference will no doubt be observed between the ratios 10·65 and 11·05, belonging respectively to solution of carbonate of sodium, which does not admit of being blown into bubbles, and to solution of resin-soap which does yield bubbles up to a certain diameter. But this, again, is a consequence of our theory; in fact, according to our Tables, the superficial viscosity is smaller in the second of these liquids than in the first, and, as I have stated above, the ratio at which the formation of bubbles first becomes possible is higher the greater the superficial viscosity. It is therefore intelligible that, if the ratio 11·05 for resin-soap allows of the formation of bubbles of moderate size, this same ratio (and still less the somewhat smaller ratio 10·65) will not allow of the formation of bubbles in solution of carbonate of sodium.

Lastly, my theory leads me to a complete explanation of the long persistence of bubbles blown with the glycerine-solution, as well as of the singular property possessed by the film which forms them of not diminishing in thickness beyond a certain degree, and then increasing in thickness again. In the first place, I endeavour to find the approximate value of the superficial viscosity of the liquid in question, and I find it equal to 80·25, whence it will be seen that it is distinctly less than that of water; the tension of the films is the same as for solution of soap, namely 5·64; hence for the ratio of these two elements in the glycerine-solution we have the number 14·22. Bearing in mind the comparatively low value of the superficial viscosity of the glycerine-solution, this ratio may be looked upon as high, and is much greater than is needful for the formation of bubbles; accordingly the glycerine-solution yields very large bubbles.

But this liquid absorbs moisture from the air, and consequently, when a bubble has been blown with it, the film is subject to two opposite influences—namely, that of weight which tends to make it thinner, and that of absorption, which tends to thicken it. The former predominatesat first, and the film gets thinner; but the descent of the liquid becomes slower through two causes—first, the diminution of the mass, and, secondly, the gradual absorption of moisture, which renders the liquid more aqueous and thus approximates its viscosity to that of water. It follows that soon the descent of the liquid becomes so slow that the augmentation of thickness due to absorption predominates. As regards the tension, M. Dupré has found that in solution of soap it varies extremely little with the proportion of water; and this probably holds good for the glycerine-solution also.

Thus, on the one hand, in consequence of the continual absorption of aqueous vapour, the film can never at any phase of its existence become very thin; and, on the other hand, the ratio between superficial viscosity and tension remains great enough to render the rupture of the film difficult, until the proportion of water assimilated by it has become very great.

I conclude by showing that in relation to the ready development of large films and the persistence of them, the part played by cohesion is subsidiary to that played by internal viscosity. In fact, for different liquids, the cohesion is known to vary in the same direction as the coefficient of the sum of the curvatures in the expression for the capillary pressure—a coefficient which, according to the researches of M. Hagen and M. Dupré, is nothing else than the tension; and since this latter is much weaker in soap-water than in pure water, the same is necessarily true for the cohesion also; but, notwithstanding, solution of soap yields enormous bubbles, while water does not yield any.

LV. *Note on a Theory of Condensed Ammonia Compounds.* *By* William Odling, *M.B., F.R.S.**

The unit of ammonia, NH^3, has the well-known property of combining with the unit of hydrochloric acid, HCl, to form a unit of the more complex body sal-ammoniac, HCl,NH^3.

Hypothetical methylene being regarded as the analogue of ammonia, chloride of methyle will be the hydrochloride of methylene, corresponding to sal-ammoniac or hydrochloride of ammonia,

$$HCl,CH^2, \quad HCl,NH^3.$$

But this chloride of methyle or hydrochloride of methylene is known to be the first term of a series of compounds, the earlier terms of which are formulated below. In a parallel column are written the formulæ of what, if they existed, would form a similar series of sal-ammoniac compounds:—

Chloride of	methyle	$HCl, C\ H^2$	$HCl, N\ H^3$
,,	ethyle	$HCl, C^2 H^4$	$HCl, N^2 H^6$
,,	propyle	$HCl, C^3 H^6$	$HCl, N^3 H^9$
,,	butyle	$HCl, C^4 H^8$	$HCl, N^4 H^{12}$
,,	amyle	$HCl, C^5 H^{10}$	$HCl, N^5 H^{15}$
		&c.	&c.

Substituting an equivalent of metallic chloride for chloride of hydrogen in the sal-ammoniac series, we have the following

* Communicated by the Author.

compounds, all of which, and many like them, are fairly well known:—

$$\frac{Zn}{2}Cl, N\ H^3,$$

$$AgCl, N^2H^6,$$

$$\frac{Cu}{2}Cl, N^3H^9,$$

$$\frac{Ca}{2}Cl, N^4H^{12}.$$

Chemists who express the composition of the chlorides of ethyle and butyle as underneath, may express the composition of the ammoniated chlorides of silver and calcium in a similar fashion; thus—

$$\begin{array}{r} Cl, C\,H^2 \\ | \\ H, C\,H^2 \end{array} \qquad \begin{array}{r} Cl, C\,H^2 \\ | \\ C\,H^2 \\ | \\ C\,H^2 \\ | \\ H, C\,H^2 \end{array} \quad \Bigg| \quad \begin{array}{r} Cl, N\,H^3 \\ | \\ Ag, N\,H^3 \end{array} \qquad \begin{array}{r} Cl, N\,H^3 \\ | \\ N\,H^3 \\ | \\ N\,H^3 \\ | \\ \frac{Ca}{2}, N\,H^3 \end{array}$$

The polyammoniated salts are all more or less unstable. It is observable, however, that the diammonia compounds are habitually less unstable than their more highly ammoniated congeners, and coincidently that in the diammonia compounds alone is it possible for each unit of ammonia to be combined directly with a constituent of the hydrochloric acid or of its representative metallic chloride.

The superior solubility of diammonia compounds is especially recognizable in the case of the best-characterized metal-ammonia bases, such as platinamine and platosamine. In the salts which these and such like bases form with hydrochloric acid, a portion of the hydrogen of the ammonia, instead of the hydrogen of the hydrochloric acid, would appear to be replaced by its equivalent of metal.

Still employing the equivalent method of notation, hydrochloride of platosamine (the yellow salt) would be represented thus:

$$HCl, NH^2\frac{Pt}{2}.$$

This salt very readily absorbs another unit of ammonia, and thereby forms the hydrochloride of diplatosamine,

$$HCl, N^2H^5\frac{Pt}{2}, \quad \text{or} \quad \begin{array}{r} H, NH^2\frac{Pt}{2} \\ | \\ Cl, NH^3 \end{array},$$

from which, as is well known, ammonia is not liberable by treatment of the salt with potash, or by its desiccation at upwards of 100°. The base $N^2 H^5 \frac{Pt}{2}$, though not procurable in the free state, as upon the above view of the cause of its stability it scarcely should be, is yet transferable from one salt to another by double decomposition with almost as much facility as ammonia itself.

What I conceive to be the constitution of the different platinous and platinic ammonia compounds in relation to each other, is indicated in the last chapter of my 'Outlines of Chemistry,' just published.

It is observable that in no stable metallicized ammonia hydrochloride is the number of nitrogen atoms more than double the number of chlorine atoms in the salt. Thus the empirical formulæ of the purpuro-cobaltic and luteo-cobaltic chlorides are $Co^2 Cl^6, 10NH^3$, and $Co^2 Cl^6, 12NH^3$ respectively. These expressions are of course easily translatable into forms harmonizing with the above suggested view of the constitution of condensed ammonia compounds.

LVI. *Notices respecting New Books.*

Methods of teaching Arithmetic. A Lecture addressed to the London Association of Schoolmistresses. By J. G. Fitch, *M.A.* Pp. 31. London, 1869.

The School Arithmetic. By J. Cornwell, *Ph.D., and* J. G. Fitch, *M.A.* Pp. 144. Tenth edition. London, 1869.

The Science of Arithmetic. By J. Cornwell, *Ph.D., and* J. G. Fitch, *M.A.* Twelfth edition. Pp. 372. London, 1868.

WE have put these books together at the head of a short notice on account of their common authorship, and of their being more or less supplementary to each other. The first of them (the lecture on methods of teaching arithmetic) contains many hints and remarks likely to be useful to the audience to which it was addressed. The point most dwelt on is the need of making learners understand the ultimate reasons of the rules for performing the elementary operations of arithmetic, such as the rules for multiplication and division of integers. We doubt whether the importance of this point is not somewhat exaggerated. Any ordinary child of nine or ten years can be brought to divide, for instance, 5382 by 23 correctly, and be made to understand what is meant by the answer, viz. that if 5382 marbles were divided equally between 23 boys, each boy would get 234 marbles. But to make the child understand each separate step of the process of the division is quite another matter. And though much can be done by a good teacher by means of a discussion of particular examples, yet we question whether any but a few ex-

ceptional children of the above age could be brought to know much more about long division than that it is a process leading to a certain result. Nor does this to any serious extent diminish the value of the intellectual training which a child goes through in the study of arithmetic. That training is undergone by means of particular examples. Thus, let the question proposed be this:—"A watch gains uniformly 13 seconds a day. It is 2 minutes 10 seconds slow on a certain day, by how much will it be fast at the end of three weeks?" The reasoning by which a child arrives at the answer is quite independent of his knowledge of the ultimate reasons of the processes of multiplication &c. that he employs.

We suppose that in reality Mr. Fitch's opinion is not very different from ours; for we find that in the book for children, of which he is the joint author (the 'School Arithmetic'), no more is attempted than the statement and illustration of rules. The method of the book is this:—In each section a typical example is given and its solution reasoned out step by step; then follow a general rule, another example worked out by the rule, and finally many examples of the rule are given for practice. Of the examples some are such as can be worked mentally, others, involving larger numbers, are to be worked on slate or paper. This classification of the examples seems to us a very valuable feature of the book; and the work altogether seems a very good school arithmetic. If we were to hint a fault, it would be that, to secure cheapness, a paper and type are used likely to prove hurtful to young eyes.

The third work on the list (the 'Science of Arithmetic') is one of more pretensions. It aims at imparting a systematic acquaintance with the principles as well as the rules of arithmetic. The authors have evidently bestowed much labour and thought upon the work, and have produced a book from which a teacher of arithmetic would doubtless learn much. The characteristic defect of the book is a want of precision of statement, which sometimes contrasts quite curiously with the air of laborious and systematic accuracy which pervades the book: *e. g.* the authors mark out nineteen arithmetical facts as *axioms*. Now, if we are justified in demanding precision in any statement, it is in an axiom; yet here is one, Axiom XV. p. 85:—"If the dividend and divisor be either both increased or both diminished the same number of times, the quotient remains unaltered." What the authors *intend* is pretty plain; but if they were held to what they *say*, it would follow that the quotient of 12 divided by 6 might be the same as that of 9 divided by 3. In short, numbers may be increased or diminished in other ways than by taking equimultiples of both or dividing both by a common factor, which is what they mean by increasing or diminishing the dividend and divisor a certain number of times. This is by no means a solitary instance of an inexactness which seriously diminishes the value of a book in many respects well executed.

LVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 399.]

June 17, 1869.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read:—

"Additional Observations on Hydrogenium." By Thomas Graham, F.R.S., Master of the Mint.

From the elongation of a palladium wire, caused by the occlusion of hydrogen, the density of hydrogenium was inferred to be a little under 2. But it is now to be remarked that another number of half that amount may be deduced with equal probability from the same experimental data. This double result is a consequence of the singular permanent shortening of the palladium wire observed after the expulsion of hydrogen. In a particular observation formerly described, for instance, a wire of 609·14 millims. increased in length to 618·92 millims. when charged with hydrogen, and fell to 599·44 millims. when the hydrogen was extracted. The elongation was 9·78 millims., and the absolute shortening or retraction 9·7 millims., making the extreme difference in length 19·48 millims. The elongation and retraction would appear, indeed, to be equal in amount. Now it is by no means impossible that the volume added to the wire by the hydrogenium is represented by the elongation and retraction taken together, and not by the elongation alone, as hitherto assumed. It is only necessary to suppose that the retraction of the palladium molecules takes place the moment the hydrogen is first absorbed, instead of being deferred till the latter is expelled; for the righting of the particles of the palladium wire (which are in a state of excessive tension in the direction of the length of the wire) may as well take place in the act of the absorption of the hydrogen as in the expulsion of that element. It may indeed appear most probable in the abstract that the mobility of the palladium particle is determined by the first entrance of the hydrogen. The hydrogenium will then be assumed to occupy double the space previously allotted to it, and the density of the metal will be reduced to one half of the former estimate. In the experiment referred to, the volume of hydrogenium in the alloy will rise from 4·68 per cent. to 9·36 per cent., and the density of hydrogenium will fall from 1·708 to 0·854, according to the new calculation. In a series of four observations upon the same wire, previously recorded, the whole retractions rather exceeded the whole elongations, the first amounting to 23·99 millims., and the last to 21·38 millims. Their united amount would justify a still greater reduction in the density of hydrogenium, namely to 0·8051.

The first experiment, however, in hydrogenating any palladium wire appears to be the most uniform in its results. The expulsion of the hydrogen afterwards by heat always injures the structure of the wire more or less, and probably affects the regularity of the expansion afterwards in different directions. The equality of the expansion and the retraction in a first experiment appears also to be

a matter of certainty. This is a curious molecular fact, of which we are unable as yet to see the full import. In illustration, another experiment upon a pure palladium wire may be detailed. This wire, which was new, took up a full charge of hydrogen, namely 956·3 volumes, and increased in length from 609·585 to 619·354 millims. The elongation was therefore 9·769 millims. With the expulsion of the hydrogen afterwards, the wire was permanently shortened to 600·115 millims. It thus fell 9·470 millims. below its normal or first length. The elongation and retraction are here within 0·3 millim. of equality. The two changes taken together amount to 19·239 millims., and their sum represents the increase of the wire in length due to the addition of hydrogenium. It represents a linear expansion of 3·205 on 100, with a cubic expansion of 9·827 on 100. The composition of the wire comes to be represented as being,

		In volume.
Palladium	100·000 or	90·895
Hydrogenium	9·827 or	9·105
	109·827 or	100·000

The specific gravity of the palladium was 12·3, the weight of the wire 1·554 grm., and its volume 0·126 cub. centim. The occluded hydrogen measured 120·5 cub. centims. The weight of the same would be 0·0108 grm., and the volume of the hydrogenium 0·012382 cub. centim. (100 : 9·827 : : 0·126 : 0·01238). The density of the hydrogenium is therefore

$$\frac{0{\cdot}0108}{0{\cdot}01238}=0{\cdot}872.$$

This is a near approach to the preceding result, 0·854. Calculated on the old method, the last experiment would give a density of 1·708.

It was incidentally observed on a former occasion that palladium alloyed with silver continues to occlude hydrogen. This property is now found to belong generally to palladium alloys when the second metal does not much exceed one half of the mixture. These alloys are all enlarged in dimensions when they acquire hydrogenium. It was interesting to perceive that the expansion was greater than happens to pure palladium (about twice as much), and that, on afterwards expelling the hydrogen by heat, the fixed alloy returned to its original length *without any further shortening of the wire.* The embarrassing retraction of the palladium has, in fact, disappeared.

The fusion of the alloys employed was kindly effected for me by Messrs. Matthey and Sellon—when the proportion of palladium was considerable, by the instrumentality of M. Deville's gas-furnace (in which coal-gas is burned with pure oxygen), or by means of a coke-furnace when the metals yielded to a moderate temperature. The alloy was always drawn out into wire if possible; but if not sufficiently ductile, it was extended by rolling into the form of a thin ribbon. The elongation caused by the addition of hydrogenium was ascertained by measuring the wire or ribbon stretched over a graduated scale, as in the former experiments.

1. *Palladium, Platinum, and Hydrogenium.*—Palladium was fused

with platinum, a metal of its own class, and gave an alloy consisting, according to analysis, of 76·03 parts of the former and 23·97 parts of the latter. This alloy was very malleable and ductile; its specific gravity was 12·64. Like pure palladium, it absorbed hydrogen, evolved on its surface in the acid fluid of the galvanometer, with great avidity.

A wire 601·845 millims. in length (23·69 inches) was increased to 618·288 millims., on occluding 701·9 volumes of hydrogen gas measured at 0° C. and 0·760 barom. This is a linear elongation of 16·443 millims. (0·6472 inch), or 2·732 on a length of 100. It corresponds with a cubic expansion of 8·423 volumes on 100 volumes; and the product may be represented—

	In volume.	
Fixed metals	100·000 or	92·225
Hydrogenium	8·423 or	7·775
	108·423 or	100·000

The elements for the calculation of the density of hydrogenium are the following, the assumption being made as formerly, that the metals are united without condensation:—

Original weight of the wire 4·722 grms.
Original volume of the wire 0·373 cub. centim.
Volume of the hydrogen extracted 264·5 cub. centims.
Weight of the hydrogen extracted, by calculation, 0·0237 grm.

The volume of the hydrogenium will be to the volume of the wire (0·373 cub. centim.) as 100 is to 8·423—that is, 0·03141 cub. centim. Finally, dividing the weight of the hydrogenium by its bulk, 0·0237 by 0·03141, the density of hydrogenium is found to be 0·7545.

On expelling all hydrogen from the wire at a red heat, the latter returned to its first dimensions as exactly as could be measured. The platinum present appears to sustain the palladium, so that no retraction of that metal is allowed to take place. This alloy therefore displays the true increase of volume following the acquisition of hydrogenium, without the singular complication of the retraction of the fixed metal. It now appears clear that the retraction of pure palladium must occur on the first entrance of hydrogen into the metal; the elongation of the wire due to the hydrogenium is negatived thereby to the extent of about one half, and the apparent bulk of the hydrogenium is reduced to the same extent; hydrogenium came in consequence to be represented of double its true density.

The compound alloy returns to its original density (12·64) upon the expulsion of the hydrogen, showing that hydrogen leaves without producing porosity in the metal. No absorptive power for vapours, like that of charcoal, was acquired.

A wire of the present alloy, and another of pure palladium, were charged with hydrogen, and the diameters of both measured by a micrometer. The wire of alloy increased sensibly more in thickness than the pure palladium, about twice as much; the reason is, that

the latter while expanding retracts in length at the same time. The expansion of both wires may be familiarly compared to the enlargement of the body of a leech on absorbing blood. The enlargement is uniform in all dimensions with the palladium-platinum alloy; the leech becomes larger, but remains symmetrical. But the retraction in the pure palladium wire has its analogy in a muscular contraction of the leech, by which its body becomes shorter but thicker in a corresponding measure.

The same wire of palladium and platinum, charged a second time with hydrogen, underwent an increase in length from 601·845 to 618·2, or sensibly the same as before. The gas measured 258·0 cub. centims., or 619·6 times the volume of the wire. The product may be represented as consisting of

	By volume.
Fixed metals	92·272
Hydrogenium	7·728
	100·000

The density of hydrogenium deducible from this experiment is 0·7401. The mean of the two experiments is 0·7473.

2. *Palladium, Gold, and Hydrogenium.*—Palladium fused with gold formed a malleable alloy, consisting of 75·21 parts of the former and 24·79 parts of the latter, of a white colour, which could be drawn into wire. Its specific gravity was 13·1. Of this wire 601·85 millims. occluded 464·2 volumes of hydrogen with an increase in length of 11·5 millims. This is a linear elongation of 1·91 on 100, and a cubic expansion of 5·84 on 100. The resulting composition was therefore as follows:—

	In volume.		
Alloy of palladium and gold	100	or	94·48
Hydrogenium	5·84	or	5·52
	105·84		100·00

The weight of the wire was 5·334 grms.

The volume of the wire was 0·4071 cub. centim.

The volume of hydrogen extracted, 189·0 cub. centims.

The weight of the hydrogen, 0·01693 grm.

The volume of the hydrogenium, 0·02378 cub. centim.

Consequently the density of the hydrogenium is 0·711.

The wire returned to its original length after the extraction of the hydrogen, and there was no retraction.

The results of a second experiment on the same wire were almost identical with the preceding.

The elongation on 601·85 millims. of wire was 11·45 millims., with the occlusion of 463·7 volumes of hydrogen. This is a linear expansion of 1·902 on 100, and a cubic expansion of 5·81 on 100. The volume of hydrogen gas extracted was 188·8 cub. centims., of which the weight is 0·016916 grm. The volume of the hydrogenium was 0·02365 cub. centim., that of the palladium-gold alloy being 0·4071 cub. centim. Hence the density of the hydrogenium is 0·715.

In a third experiment made on a shorter length of the same wire, namely 241·2 millims., the amount of gas occluded was very

similar, namely 468 volumes, and was not increased by protracting the exposure of the wire for the long period of twenty hours. There can be little doubt, then, of the uniformity of the hydrogenium combination, the volumes of gas occluded in the three experiments being 464·2, 463·7, and 468 volumes. The linear expansion was 1·9 on 100 in the third experiment, and therefore similar also to the preceding experiments.

The hydrogenium may be supposed to be in direct combination with the palladium only, as gold by itself shows no attraction for the former element. In the first experiment the hydrogenium is in the proportion of 0·3151 to 100 palladium and gold together. This gives 0·3939 hydrogenium to 100 palladium; while a whole equivalent of hydrogenium is 0·939 to 100 palladium*. The hydrogenium found is by calculation 0·4195 equivalent, or 1 equivalent hydrogenium to 2·383 equivalents palladium, which comes nearer to 2 equivalents of the former with 5 of the latter than to any other proportion.

To ascertain the smallest proportion of gold which prevents retraction, an alloy was made by fusing 7 parts of that metal with 93 parts of palladium, which had a specific gravity of 13·05. The button was rolled into a thin strip and charged with hydrogen by the wet method. An occlusion of 585·44 volumes of gas took place, with a linear expansion of 1·7 on 100. A retraction followed to nearly the same extent on afterwards expelling the hydrogen by heat.

With another alloy, produced by fusing 10 of gold with 90 of palladium, the occlusion of gas was 475 volumes, the linear expansion 1·65 on 100. The retraction on expelling the gas afterwards was extremely slight. To nullify the retraction of the palladium, about 10 per cent. of gold appears therefore to be required in the alloy.

Another alloy of palladium of sp. gr. 13·1, and containing 24·79 per cent. of gold, underwent no retraction on losing hydrogen, as already stated.

The presence of so much gold in the alloy as half its weight did not materially reduce the occluding power of the palladium. Such an alloy was capable of holding 459·9 times its volume of hydrogen, with a linear expansion of 1·67 per cent.

3. *Palladium, Silver, and Hydrogenium.*—The occluding power of palladium appeared to be entirely lost when that metal was alloyed with much more than its own weight of any fixed metal. Palladium alloys containing 80, 75, and 70 per cent. of silver occluded no hydrogen whatever.

With about 50 per cent. of silver, palladium rolled into a thin strip occluded 400·6 volumes of hydrogen. It expanded 1·64 part in 100 in length, and returned to its original dimensions without retraction upon the expulsion of the gas. The specific gravity of this silver-palladium alloy was 11·8; the density of the hydrogenium 0·727.

An alloy which was formed of 66 parts of palladium and 34 parts of silver had the specific gravity 11·45. It was drawn into wire and found to absorb 511·37 volumes of hydrogen. The length of the wire increased from 609·601 to 619·532 millims. This is a linear

* $H=1$: $Pd=106·5$.

elongation of 1·629 on 100, or cubic expansion of 4·97 on 100. The weight of the wire was 3·483 grms., its volume 0·3041 cub. centim. The absolute volume of occluded hydrogen was 125·1 cub. centims., of which the weight is 0·01120896. The volume of the hydrogenium was 0·015105 cub. centim. The resulting density of hydrogenium is 0·742.

In a repetition of the experiment upon another portion of the same wire, 407·7 volumes of hydrogen were occluded, and the wire increased in length from 609·601 millims. to 619·44 millims. This is a linear expansion of 1·614 part on 100, and a cubic expansion of 4·92 on 100. The absolute volume of hydrogen gas occluded was 124·0 cub. centims., and its calculated weight 0·01111 grm. The volume of the hydrogenium being 0·1496 cub. centim., the density of hydrogenium indicated is 0·741. The two experiments are indeed almost identical. The wire returned in both experiments to its original length exactly, after the extraction of the gas.

4. *Palladium, Nickel, and Hydrogenium.*—The alloy, consisting of equal parts of palladium and nickel, was white, hard, and readily extensible. Its specific gravity was 11·22. This alloy occluded 69·76 volumes of hydrogen, with a linear expansion of 0·2 per cent. It suffered no retraction below its normal length on the expulsion of the gas by heat.

An alloy of equal parts of *bismuth* and palladium was a brittle mass that did not admit of being rolled. It occluded no hydrogen, after exposure to that gas as the negative electrode in an acid fluid for a period of 18 hours. It seems probable that malleability and the colloid character, which are wanting in this bismuth alloy, are essential to the occlusion of hydrogen by a palladium alloy.

An alloy of 1 part of *copper* and 6 parts of palladium proved moderately extensible, but absorbed no sensible amount of hydrogen. The metallic laminæ which remain on digesting this alloy in hydrochloric acid, and which were found by M. Debray to be a definite alloy of palladium and copper (Pd Cu), exhibited no sensible occluding power.

The conclusions suggested as to the density of hydrogenium, by the compound with palladium alone and by the compounds with palladium alloys, are as follows:—

	Density of Hydrogenium observed.
When united with palladium	0·854 to 0·872
When united with palladium and platinum	0·7401 to 0·7545
When united with palladium and gold	0·711 to 0·715
When united with palladium and silver	0·727 to 0·742

The results, it will be observed, are most uniform with the compound alloys, in which retraction is avoided; and they lie between 0·711 and 0·7545. It may be argued that hydrogenium is likely to be condensed somewhat in combination, and that consequently the smallest number (0·711) is likely to be the nearest to the truth. But the mean of the two extreme numbers will probably be admitted as a more legitimate deduction from the experiments on the com-

pound alloys, and 0·733 be accepted provisionally as the approximate density of hydrogenium.

I have the pleasure to repeat my acknowledgments to Mr. W. C. Roberts for his valuable assistance in this inquiry.

Could the density of hydrogenium be more exactly determined, it would be interesting to compare its atomic volume with the atomic volumes of other metals. With the imperfect information we possess, one or two points may be still worthy of notice. It will be observed that palladium is 16·78 times as dense as hydrogenium taken as 0·733, and 17·3 times as dense as hydrogenium taken as 0·711. Hence, as the equivalent of palladium is 106·5, the atomic volume of palladium is 6·342 times as great as the atomic volume of hydrogenium having the first density mentioned, and 6·156 as great with the second density. To give an atomic volume to palladium exactly six times that of hydrogenium, the latter element would require to have the density 0·693.

Taking the density of hydrogenium at 0·7, and its atomic volume equal to 1, then the following results may be deduced by calculation. The atomic volume of lithium is found to be 0·826; or it is less even than that of hydrogenium (1). The atomic volume of iron is 5·026, of magnesium 4·827, of copper 4·976, of manganese 4·81, and of nickel 4·67. Of these five metals, the atomic volume is nearly 5 times that of hydrogenium. Palladium has already appeared to be nearly 6 times. The atomic volume of aluminium on the same scale is 7·39, of sodium 16·56, and of potassium 31·63.

GEOLOGICAL SOCIETY.

[Continued from p. 403.]

April 14th, 1869.—Prof. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—

3. "On the Salt-mines of St. Domingo." By F. Ruschhaupe. Communicated by Sir R. I. Murchison, Bart., F.P.G.S.

The author described the Cerro de Sal, or Salt Mountain of St. Domingo. It extends about 3 leagues in length, and consists, according to the author, of rocks "of the Red Sandstone class"—which, where the chief visible deposits of salt occur, are principally gypsum schists, sometimes very argillaceous. The salt is generally surrounded by an ash-like mass consisting of gypsum and clay. The author compared the gypsum beds with those of the Keuper. The beds are thrown into a perpendicular position, and the same change is observable for miles in the Savannas. An immense body of salt, 250–300 feet broad, is exposed upon the north side of the mountain. The salt is very white and pure, and might easily be conveyed to the port of Barahona, about 18 miles distant.

4. "A description of the 'Broads' of East Norfolk, showing their origin, position, and formation in the Valleys of the Rivers Bure, Yare, and Waveney." By B. B. Grantham, Esq., C.E., F.G.S.

The author described the general characters of the "Broads," or shallow lakes of East Norfolk, and indicated their connexion with the river-valleys. He regarded them as the last traces of great estuaries, now cut off from the influence of the sea by upheaval.

5. "On a peculiar instance of Intraglacial Erosion near Norwich." By Searles Wood, Jun., Esq., F.G.S., and F. W. Harmer, Esq.

The authors described the general structure of the valley of the Yare near Norwich, in which the fundamental chalk-rock is covered by the following drift-beds:—1, the Chillesford sand and clay; 2, pebbly sands and pebble-beds; 3, the equivalent of the contorted Drift of Cromer; 4, the middle glacial sand; and 5, the Boulder-clay. The valley is hollowed out in these beds. Sewer-shafts sunk in the bottom of the valley near Norwich have shown the existence of an abrupt hole or narrow trough in the chalk, having one of its sides apparently perpendicular. This is filled up in part by a deposit of dark-blue clay, full of chalk débris, exactly resembling the Boulder-clay at a distance from Norwich, but quite different in character from that occurring in the vicinity (No. 5); and this is overlain in part by a bed of the middle glacial sand (No. 4), and in part by a postglacial gravel. The authors believed that this peculiar hole or trough was excavated by glacial action after the deposition of the bed No. 3, and that it belongs to the earliest part of the middle glacial period. At Sommerleyton Brick-kiln, near Lowestoft, a perfectly similar bed occurs between the drift and sand (Nos. 3 and 4.

6. "On the Lignite-mines of Podnernuovo, near Volterra." By E. J. Beor, Esq., F.G.S.

The author states that the deposit of Lignite at Podnernuovo, near Volterra, is of lacustrine origin, and consists of two parallel strata of compact coal about $2\frac{1}{2}$ metres (=8 feet 4 in.) in thickness, separated by a thin stratum of marl, with marl-shells. The lower coal-bed lies on a bed of marl with marsh-shells, and the upper bed is covered by a marine formation belonging to the Upper Miocene. The lignite comes to the surface near the Alberese, where it extends for a considerable distance. Some shifts occur, bringing the upper bed down nearly to the level of the lower one; the inclination of the beds diminishes gradually; and the intervening stratum of marl decreases in thickness, and probably at last thins out altogether. The coal in the upper bed is better than that in the lower one. The author remarks that this lignite deposit differs from those of the neighbouring valleys in being purely of marsh origin, while they are estuarine.

April 28th, 1869.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Geology and Mineralogy of Hastings County, Canada West." By T. C. Wallbridge, Esq.

Before describing the gold and iron-ores of Hastings, which formed the main subject of this paper, the author introduced a general sketch of the geology of the county. After noticing certain local

deposits of recent origin, he described the extensive accumulations of drift-gravels and boulder-clay. A single boulder near the Shannonville railway-station was said to cover an area of about 5 acres, and to have a thickness of 100 feet. The evidences of glacial action over the whole country were referred to, and the direction of ice-marks cited from several localities. Below the posttertiary deposits the rocks consist, in the southern townships, of Lower Silurian limestones referred for the most part to the Trenton group, and, in the northern townships, of a large series of metamorphic rocks, supposed to be of Lower Laurentian age. Bosses of syenite and gneiss penetrate the Silurian beds to the south of the main Laurentian mass; and several outliers of Trenton limestone point to the former extension of the Silurian rocks northwards. All the minerals of economic value are confined to the Laurentian area.

Gold was first discovered in the county of Hastings in 1866. The author described in detail the singular occurrence of the metal at the Richardson Mine in Madoc, where it was found in two pockets associated with a peculiar black carbonaceous substance, a ferruginous dolomite, and ochre-brown iron-ore. Assays of the surrounding rocks showed the existence of gold even at a considerable distance from the mine. Mention was also made of several other gold mines, in Madoc, Marmora, and Elzevir, from which specimens were exhibited, and analyses of ore quoted.

The iron-ores of Hastings occur partly as magnetic oxide and partly as hæmatite. In addition to the well-known "Big Ore-bed" and the "Seymour bed," the writer called attention to some new localities of magnetic ore in Madoc. The deposit of hæmatite called the "Kane Ore-bed" was discovered by the author some years back; and from ancient workings in this bed (apparently those of the Indians, who may have used the ochre as war-paint) he has obtained bone needles and other objects of human workmanship. Attention was then directed to a large deposit of specular iron-ore in Hungerford, hitherto undescribed, and to the pyrrhotine or magnetic pyrites of Madoc.

The paper concluded with a notice of the galena and other less important minerals of the county.

2. "On the distribution of Flint Implements in the Drift, with reference to some recent discoveries in Norfolk and Suffolk." By J. W. Flower, Esq., F.G.S.

The author noticed some recently discovered localities in the valley of the Little Ouse which have yielded Flint Implements, viz.:—at Broomhill, about 350 feet from and 5 or 6 feet above the level of the river; at Gravel Hill, about 1 mile from and 10 feet above the river; at Shrub Hill, about 1 mile from and only a foot or two above the river; and at Lakenheath, nearly 3 miles from the river, and 60 feet above it. In the first three of these localities the worked flints are in coarse gravel, resting immediately on the Cretaceous beds (chalk in the first and second, gault in the third), and overlain by regular deposits of gravel and sand. The implements resemble those of Acheul, Thetford, and Salisbury, but present some peculiarities, from which the author inferred that each place had its own

workmen, and that the different forms were intended to answer different purposes. At Brandon, implements formed of quartzite were found in a bed consisting of rounded quartzite pebbles mixed with about one-fourth of flints. Flint implements occurred beneath this bed.

The author indicated the geographical characters of the district and the peculiarities in the distribution of the flint implements, which he regarded as in accordance with the phenomena presented by the valley of the Somme; and he argued from the consideration of all the facts that the implements were not transported to their present situation by the agency of the rivers in whose valleys they occur, but that they were made upon the spot, exposed upon the surface with the gravels in which they are found and from which they were made, and finally covered up by the river-gravels and sandy beds which now overlie them.

LVIII. *Intelligence and Miscellaneous Articles.*

ON THE EXTENSION OF LIQUIDS UPON EACH OTHER. BY R. LUDTGE.

When a drop of liquid is placed on the surface of another liquid with which it does not mix, either the drop may retain the shape of a lens floating on this liquid, or it may spread out and form a very thin layer. The first case is that of a drop of water placed upon oil, or of a drop of oil upon alcohol; the second that of oil upon water, or of alcohol on glycerine.

It is readily ascertained that the thickness of the liquid on which is placed the drop of the second substance has an influence on the extension of this drop on its surface. If this thickness is adequate (at least 1 centim.), the drop readily expands, forming a very thin layer, too thin indeed to produce the phenomenon of coloured rings. When it is very small (1 to 5 millims. and even less), the drop in extending hollows in its centre the liquid surface, to such an extent sometimes as to moisten the bottom of the vessel in which the surface was contained, by driving away at this point the liquid which originally covered it. The nature of the material of which the vessel is made has no influence on the relative positions which the two liquids assume under these circumstances; it does not seem to depend on any difference in the force with which the two liquids adhere to the bottom.

M. Ludtge brings this out more clearly by the following experiment, in which he quite gets rid of the vessel, so that adhesion cannot come into play. On a lamina of oil produced in a circular iron wire frame, he places a drop of soap-water; there is thus formed a circular lamina of soap-water which gradually extends into the interior of the lamina of oil until it fills the entire ring, while the oil is repelled in the form of small droplets which adhere to the iron wire. A lamina of water may also first be produced in the ring; this may be driven away by a drop of oil delicately placed upon it, which spreads over the frame in its place; and this lamina of oil may finally be replaced by another of soap-water, as we have seen. We might obvi-

ously work in this way with all substances which are capable of spreading over each other, were it not that there are some which cannot be made to form a thin plate on a framework. In the case of these liquids, the experiment is made by replacing the free lamina by one almost as thin and as stretched, which is formed by letting the liquid extend on a carefully cleaned glass plate.

One of the two substances may be extended as a thin lamina on another liquid, and the lamina thus produced may be worked with like a free one. These two latter methods have this advantage over the use of a skeleton, that the surface of contact between the two liquids is smaller, and that they mix or combine less easily; thus the experiment is in many cases greatly facilitated.

The author has investigated a great number of substances from this point of view. He has found it to be an extremely general fact, and that there is probably no liquid, excepting perhaps mercury, which has not the property of spreading as a thin lamina on a great number of liquids, and in regard to which other substances do not enjoy the same property. The following are the principal results to which this investigation has led.

1. When one liquid can extend in a thin lamina upon the surface of another liquid, the second can never extend in the same way over the first.

2. Two liquids whose reciprocal adhesion is greater than the cohesion of that one of them in which this property is smallest, have always the property that a drop of the one with the smaller cohesion extends upon the other.

3. A drop of the latter retains its shape when placed on the surface of the former, and becomes coated with a thin layer of the first liquid.

4. All liquids which satisfy the above conditions as to the magnitude of adhesion, may be arranged in a series in which each antecedent liquid spreads on the surface of a succeeding one, and never conversely.

5. This series is the same as that obtained when the same liquids are arranged in the order of their capillarity-constants

$$\left(\frac{g}{2r\pi}=\frac{\mathrm{H}}{2}=\frac{a^2}{2}\epsilon g=\mathrm{T}=\alpha\right),$$

the smallest constant being first.

6. The rapidity with which this extension takes place is almost proportional to the interval which separates them in the Table.

7. The phenomenon is the more distinct the less the miscibility of two liquids and the greater the difference of their cohesions.

8. The extension of a liquid on its own surface may be effected by placing a drop at a high temperature upon the surface of the liquid at a lower temperature.

9. The greater the cohesion of a liquid the more difficult is it to obtain a clean surface. This is the case with water for instance, on which almost all liquids can extend.

The substances on which the author has worked are the following,

arranged in such an order that each can extend a thin lamina on a following one; it will be seen by the numbers that the order is the same as that for the capillarity, the authority for which is given:—

Ether	1·89	Frankenheim.
Acetic ether	2·29	Frankenheim.
Alcohol	2·49	Frankenheim.
Benzole	2·78	Frankenheim.
Essence of turpentine	2·78	Frankenheim.
Soap-water	2·8	Plateau.
Acetic acid	2·884	Bede.
Oil of poppies	3·05	Guthrie.
Bisulphide of carbon	3·31	Guthrie.
Solution of potash		
Glycerine	4	Plateau.
Nitric acid	6·026	Frankenheim.
Sulphuric acid	6·623	Frankenheim.
Hydrochloric acid	7·026	Frankenheim.
Ammonia		
Sulphate of copper		
Water	7·58	Frankenheim.
Chloride of ammonium		
Solution of chloride of iron.		

—Poggendorff's *Annalen*, No. 7, 1869; *Bibliothèque Universelle de Genève*, September 15, 1869.

MEASUREMENT OF THE ELECTRICAL CONDUCTIVITY OF LIQUIDS HITHERTO SUPPOSED TO BE INSULATORS.

To the Editors of the Philosophical Magazine and Journal.

Tamworth House, Mitcham Common, S.,
September 22, 1869.

GENTLEMEN,

You have given in the August Number of this Magazine an extract from the *Comptes Rendus* for June, on the "Measurement of the Electrical Conductivity of Liquids hitherto supposed to be Insulators." In a paper read in the Chemical Section of the British Association at Dundee, 1867, I gave the resistances, in B.A. units, of a definite length and thickness of oils, and pointed out in some instances the electrolysis resulting from the tests. This paper appeared in the Report of the British Association for 1867, the Chemical News, October 1867, and in the Proceedings of the British Pharmaceutical Conference, as well as in the Pharmaceutical Journal for October 1867.

Some of the oils operated upon gave much higher resistances than any of the liquids tested by M. Saïd-Effendi. In the case of oil of turpentine, I found by continued contact with the battery that its resistance became considerably reduced in consequence of electrolysis, and pointed out the importance of this fact to the detection of oil of turpentine when employed as an adulterant to volatile oils.

Yours obediently,

THOMAS T. P. BRUCE WARREN.

ON THE FREEZING-POINT OF WATER CONTAINING DISSOLVED GASES, AND ON THE REGELATION OF WATER. BY C. SCHULTZ.

Gases, like solids or liquids, dissolved in water lower its freezing-point. This is well known in the case of hydrochloric acid and of ammonia, which, from the exception they present to the law of the absorption of gases, are not considered to form mere solutions in water. The same effect is very distinct in the case of sulphurous and carbonic acids; and by adopting certain precautions it may also be observed in the case of the permanent gases oxygen, hydrogen, and nitrogen.

The following experiment shows that pure water solidifies at a temperature at which water containing dissolved air remains liquid. In a glass bulb provided with a U-tube, water, freed from air by boiling for a sufficient length of time, was introduced, and was shut off from communication with the atmosphere by mercury in the bend. This vessel was surrounded by melting ice obtained from distilled water. Over this melting ice a current of air washed with water was passed. The water in the bulb had, by strong cooling, been made to freeze, and the ice formed melted, except a very small piece. If the vessel is then surrounded by the mixture of aërated water and ice, large crystals of ice are gradually formed on it.

Helmholtz has given an experiment the method of which has been applied in the foregoing one. In a vacuous vessel containing water, ice is formed when it is surrounded with ice melting in the air. This experiment is designed to show that ice melting in the air has, owing to the external pressure, a lower melting-point than that which has been freed from this pressure. But it has been shown above that ice melting in the air has a lower melting-point than that which melts under the same pressure without contact with air.

By comparison with the known lowering of the melting-point of pure water produced by pressure, we are in a condition to determine the small value of the depression of the melting-point produced by absorbed air. If the open end of the U-tube in the above apparatus be connected with a column of mercury under an excess of pressure of two atmospheres, the renewed formation of ice almost ceases; and with an excess of pressure of $3\frac{1}{2}$ atmospheres the ice in the vessel gradually melts. According to Thomson, the lowering of the melting-point of pure water by a pressure of 3 atmospheres amounts to $0^\circ{\cdot}02$; so that ice in contact with water which is saturated with air under the pressure of 1 atmosphere, melts at about this much lower temperature than it does under the same pressure, air being excluded. If we define the temperature 0° as that of the melting-point of pure water under a pressure of 760 millims. mercury, the zero-point of the thermometer may, on the ordinary determination in melting ice, lie between 0 and $-\frac{1}{50}^\circ$.

The alteration in the melting-point of water by absorbed hydrogen is far smaller. Water which is saturated with hydrogen under the ordinary atmospheric pressure freezes in a mixture of ice and water saturated with air.

To investigate the influence of the quantity of the absorbed gases on the magnitude of the change in the melting-point, the temperature of a mixture of ice and water which was saturated under 1, 2, 3 atmospheres was examined, and was found to be $-0°{\cdot}13$, $-0°{\cdot}25$, and $-0°{\cdot}35$. The alteration in the melting-point seems proportional to the amount of dissolved gas.

The remarkable property which ice has of regelation has been variously interpreted. Faraday has explained it by assuming that the particles in the interior of a mass of ice have a higher melting-point than those on the surface*. Forbes† and others assume that ice on melting assumes an intermediate condition of softness, and that in this condition pieces adhere together, like those of weldable metals. Thomson ‡ and, subsequently, Helmholtz explain the phenomenon by an alteration in the melting-point of ice by pressure. There must always be an increase in pressure on intimate contact of the pieces of ice; under this pressure a portion of the ice must melt at the surface of contact, the water formed must run off, and, in virtue of its lower temperature, partially freeze again in places where it is liberated from pressure.

If in regelation a fresh formation of ice from water be assumed, the action of the air on the melting-point must influence the process of regelation. Pure ice can only retain a temperature of 0° in pure water; when it slowly thaws in air, or in water containing air, its temperature is lower; a layer of pure water, or of water which is not saturated with air, can therefore freeze between two pieces of such ice. This condition must in many cases be considered to exist.

Hence in an atmosphere of carbonic acid the phenomenon of regelation must be more decided than in common air; the experiment, in fact, frequently succeeds. Yet the rapidity with which water becomes saturated with carbonic acid seems to exert a disturbing influence; for probably the water between the surfaces in contact is also quickly saturated with carbonic acid.—Poggendorff's *Annalen*, No. 6, 1869.

DISTURBANCES OF RESPIRATION, CIRCULATION, AND OF THE PRODUCTION OF HEAT AT GREAT HEIGHTS ON MONT BLANC. BY M. LORTET.

On the 17th and 26th of August, 1869, I made two ascents of the highest peak of Mont Blanc. In the interval I twice passed the Col du Géant; and before returning to Lyons I traversed other high passes, and ascended several secondary summits in order to verify the results I had obtained in reference to the disturbance which remaining or moving at great heights may produce in various physiological functions. The instruments which I used for estimating these are the anapnograph of Bergeon and Kastus, Marey's sphygmograph,

* Proc. Roy. Soc. vol. x. p. 440.
† Phil. Mag. S. 4. vol. xvi. p. 544.
‡ Proc. Roy. Soc. vol. ix. p. 141.

and maximum thermometers with an air-bubble and index specially constructed by Baudin and which readily indicate the hundredth of a degree.

In proportion as we ascend from a low to a considerable altitude, the disturbance of the physiological functions becomes greater and greater. While it is scarcely perceptible in going from Lyons to Chamounix (that is, from a height of 656 feet to one of 3444 feet), it is very appreciable from Chamounix to the Grands-Mulets (3444 to 10,000 feet), more perceptible still from the Grands-Mulets to the Grand-Plateau of Mont Blanc (from 10,000 to 12,897 feet); lastly this disturbance becomes very appreciable from the Grand-Plateau to the Bosses-du-Dromadaire (14,944 feet) and at the summit of the Calotte of Mont Blanc (15,776 feet). We shall pass in review the variations which the respiration, the circulation, and the internal temperature of the body undergo at the different heights, either during actual walking or after a suitable time of rest.

Respiration.—From Chamounix to the Grand-Plateau (from 3444 to 12,897 feet) the disturbances of the respiration are little marked in those who are accustomed to the ascent of high mountains, who hold the head down to diminish the orifice of the respiratory organs, who merely breathe through the nasal orifice, and keep the mouth shut, taking care to suck an inert body, such as a stone. From Chamounix to the Grand-Plateau the number of respiratory motions is scarcely altered; we found twenty-four in a minute, as at Lyons and Chamounix. But from the Grand-Plateau to theBosses and thence to the top we observed thirty-six in a minute. The respiration is short and obstructed; it seems as if the pectoral muscles became rigid, and the sides squeezed in a vice. At the top, after two hours' rest, these inconveniences gradually disappear. The respiration sinks to twenty-five a minute; but it remains obstructed, and the anapnograph shows that the quantity of air inspired and expired is much less than on the plain. The air being under a very low pressure, the quantity of oxygen brought in a given time into contact with the blood is necessarily very small.

Circulation.—During the ascent, although the pace was extremely slow, the circulation was enormously accelerated. At Lyons, in a state of rest and while fasting, the mean number of the pulsations was 64 in a minute. In the ascent from Chamounix to the top of Mont Blanc this number gradually increases, according to the heights, to 80, 108, 116, 128, 136, and finally, in ascending the last ridge, which leads from the Bosses to the top, to 160 and more in a minute. These ridges are, it is true, extremely difficult; they have an inclination of from 45° to 50°; but the pace was very slow, never more than 32 paces in a minute, and frequently less. The pulse is feverish, rapid and weak. The artery is felt to be almost empty. Thus the least pressure stops the current of blood in the vessel. The blood must pass with great rapidity into the lungs, a rapidity which aggravates the bad oxygenation it already undergoes owing to the rarefaction of the air. From 14,760 feet the veins of the hands, the forearms, and the temples swell; and every one, including the

guides, feels a heaviness of the head and a somnolence which are frequently very painful, evidently due to a venous stagnation and imperfect oxygenation of the blood. Even after two hours' complete rest and while still fasting, the pulse always remains between 90 and 108. The sphygmograph applied to the wrist after an hour's rest indicates an extremely feeble tension, and a most pronounced dicrotism. According to M. Marey, this defect of tension must be due to the fact that, owing to muscular motion, the blood flows more rapidly through the small vessels. When the sphygmograph is applied to persons suffering from mountain-sickness, curves are obtained which exactly resemble those obtained in cases of algidity. The pulse is so weak that the spring of the instrument is scarcely raised. This alone would indicate a general cooling of the body.

Internal Temperature of the Body.—This was always taken with great care at different heights, the thermometer being placed in the mouth underneath the tongue; the mouth itself was closed, and breathing was effected through the nose. The thermometer was a Walferdin's maximum with index, on which, from 30° to 40°, the hundredths of a degree could be read off. The index facilitated the reading, and prevented any errors. The instrument was always left for at least fifteen minutes in the mouth, a time which was far more than sufficient for it to reach the maximum.

While fasting and exactly in the same conditions, *during the ascent*, the decrease of the internal temperature of the body is very remarkable, and *is proportional to the altitude* reached. This is easily seen by an inspection of the following Table, which condenses the observations made upon myself during my two ascents of Mont Blanc.

Temperature taken under the Tongue.

Names of the stations.	Height in feet.	Ascent on Aug. 17, 1869.		Ascent on Aug. 26, 1869.		Temperature of the air.	
		At rest.	In motion.	At rest.	In motion.	Aug. 17.	Aug. 26.
Lyons	656	36·4°	36·2°	......	......	+22·7	
Chamounix	3,144	36·5	36·3	37·0	35·3	+10·1	+12·4
Cascade-du-Dard	4,920	36·4	35·7	36·3	34·3	+11·2	+13·4
Chalet-de-la-Para	5,264	36·6	34·8	36·3	34·2	+11·8	+13·6
Pierre-pointue	6,721	36·5	33·3	36·4	33·4	+13·2	+14·1
Grands-Mulets	10,002	86·5	33·1	36·3	33·3	− 0·3	− 1·5
Grand-Plateau	12,897	36.3	32·8	36·7	32·5	− 8·2	− 6·4
Bosses-du-Dromadaire	14,944	36·4	32·2	35·7	32·3	−10·3	− 4·2
Top of Mont Blanc	15,777	36·3	32·0	36·6	31·8	− 9·1	− 3·4

It is thus seen that, during the muscular efforts of the ascent, the internal temperature of the body may be lowered in ascending from 3444 to 15,777 feet by from 4° to 6°—an enormous diminution for mammals. If we remain stationary for a few seconds, the temperature rapidly rises to very nearly its normal maximum; at the top of

Mont Blanc, however, where every one feels a little uneasiness, more than half an hour elapsed before the thermometer attained its normal height. These data cease to be true during digestion. Then, in spite of the efforts which the ascent necessitates, the temperature is maintained at about 36° or 37°, and even exceeds 37°·3. The influence of the food does not last long; scarcely half an hour after having eaten, the body is again cooled.

Whence arises this diminution of temperature? In a state of rest and while fasting man burns the materials of his blood, and the heat developed is altogether employed in keeping his temperature constant during the variations of the atmosphere. On a plain, and by mechanical efforts, the intensity of the respiratory combustions, as Gavarret has shown, increases proportionally to the expenditure of force. Heat is transformed into mechanical force; but from the density of the air and the quantity of oxygen inspired, enough heat is formed to compensate this expenditure. On a mountain, on the contrary, especially at great heights and on very steep snowy ascents, where the mechanical labour of the ascent is very great, an enormous quantity of heat must be transformed into muscular force. This expenditure of force consumes more heat than the organism can furnish; hence the body is cooled, and frequent halts must be made in order to reheat it. Although the body be burning and in a state of perspiration, it becomes cooler in ascending, because it consumes too much heat, and the respiratory combustion cannot furnish a sufficient quantity, owing to the small density of the air. It is this rarefaction that causes less oxygen to enter the lungs at an elevated place than on the plain. The rapidity of the circulation is also a cause of cooling, the blood not having sufficient time to become properly charged with oxygen. At a great height, as Gavarret has remarked, the respiratory and circulatory motions are accelerated, not only in order to render possible the absorption of a suitable quantity of oxygen, but also to remove from the blood the dissolved carbonic acid. But this gaseous exhalation, though very active, is no longer sufficient to keep up the normal composition of the blood, which remains supersaturated with carbonic acid; hence the headache, sickness, sleepiness which sometimes is almost irresistible, and the still greater cooling which affects both travellers and guides, on reaching a height of 13,000 or 14,000 feet. The *mountain-sickness*, which attacked two of my companions very severely, is especially due to this considerable cooling, and probably also to the blood being vitiated by carbonic acid. During digestion the cooling becomes almost zero; hence the usage of the guides to eat about every two hours. Unfortunately at great heights the want of appetite becomes usually so great that it is impossible to swallow any food.

The secretions exhibited nothing remarkable. The urine contained neither sugar nor albumen; but it was considerably diminished.—*Comptes Rendus*, September 20, 1869.

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END OF THE THIRTY-EIGHTH VOLUME.

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